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TABLE OF CONTENTS

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ATTITUDE IN RELATION TO THE PSYCHO-PHYSICAL JUDGMENT[1]

By S. S. George

CONTENTS

In psychophysical work the *doubtful* judgment has always given difficulty. At first it was a question how this judgment should be treated mathematically; later, when rules were established for dealing with *doubtful* cases or 'or-judgments' as intermediate categories, the question turned upon the nature of the *doubtful* judgment itself. The judgments 'greater,' 'equal,' and 'less' may be passed upon stimuli and sense-impressions; but what is there about stimuli or sense-impressions that leads them to be reported as *doubtful?* Rather

[1] From the Psychological Laboratory of Cornell University.

it would seem to be the observer who is doubtful. In admitting that judgment, do we not allow a change in the reacting organism, whose constancy is a necessary presupposition of the psychophysical determination?

This question constitutes the principal problem of the present study. We first tried to examine the *doubtful* judgment directly, and to determine whether it necessarily or even usually represents a change in the attitude of the reacting organism. It soon became evident however that we could not attack the problem successfully in this way. We therefore sought by an instruction to compel the observer to maintain a constant attitude or disposition while giving judgments. We wished to see whether or not *doubtful* judgments would occur under this instruction; and if they occurred whether they gave evidence of a dispositional shift which violated the instruction. The evidence of such a shift was to be obtained both directly, from the reports of the observers, and indirectly, from change in their reaction-times of judgment. We hoped also to secure evidence of other attitudinal changes, if they occurred, and to trace their influence. We planned further to examine the status of all the categories used by the observers under the instruction. We specifically suggested the 'or-categories,' which are often used interchangeably with *doubt,* and the category 'no-difference,' which we thought might prove to be a truly impressional judgment that could fitly replace the judgment *doubtful.*[2] And since there is no *a priori* proof that an attitudinal change is not involved in the fundamental categories, 'greater,' 'equal,' and 'less,' we sought for empirical proof of their occurrence under a constancy of disposition.[3]

We have thus aimed to distinguish between intra-serial change, expressed in the law of the psychometric function, and extra-serial change due to gross shift of the observer's attitude. The organism is never constant, and its inconstancy is measured whenever a psychophysical correlation is made; when, *e. g.,* the correlation is a limen, the measure of intra-serial change is the measure of precision, *m. v.* or *h.* But

[2] The category 'no-difference' did not, in fact, realize this logical expectation.

[3] We are obliged, for reasons of space, to omit historical references to discussions of the *doubtful* judgment (we have found but few that bear at all directly upon our problem), as well as the discussion of certain published results, suggested by the results of our own inquiry. Our general position is closely related to that of S. W. Fernberger (this JOURNAL, xxv, 1914, 538 ff.), though we have approached the problem of attitude from a different direction.

this inconstancy, the range of play within the neural machine, is different from the inconstancy which results when the machine is given a different set, and so becomes in reality another machine. Extra-serial changes lie outside of the series of categories prescribed by the experiment; they cannot be taken account of, or measured; they should therefore be, so far as possible, excluded.

The Instructions

In the preliminary work definite categories were recommended. We wished our observers to take the judgment-scale as a whole, as a single complete series limited on the one side (say) by 'greater' and on the other by 'equal.' The instruction follows:

"You are to report on the relative intensity of two sounds; the second to be judged in terms of the first. The second, in this series, will always be greater or equal; and you will therefore be required to judge in categories whose limiting judgments are greater and equal. These categories are: greater, greater or no-difference, no-difference, no-difference or equal, equal. You will be asked after every judgment to give the introspective basis of your report. You are to judge with full attention."

This procedure, however, failed to secure the desired results. It soon became clear that no one of our observers was maintaining anything like a constant attitude toward the sounds. They were rather taking every judgment by itself, unrelated to the other types of judgment. We wanted them to realize a *series* of prescribed judgments, which would lie in one and the same straight line, and to judge with respect to a *gradation* that should remain true to the shades and nuances of a single mental attitude. The instructions were therefore changed as follows:

"You are to report on the relative *intensity* of two noises: the second to be judged in terms of the first. In this series, the second stimulus will always be greater than or equal to the first.

"You are to employ judgment categories of a *serial* nature; that is, you are to keep your receptive attitude constant throughout. Such categories might be: greater, greater or no-difference, no-difference, no-difference or equal, equal, . . . less.

"You are to judge with full attention. After judgment, you will make a report to the experimenter, noting especially (1) any gross shift of attitude, and its conditions; and (2) the conditions of change from one judgment category to another, such as may occur without change of general attitude."

The instructions were varied in a 'less'-series, by substituting the word 'less' for 'greater' throughout; while in the visual series the word 'extents' was used instead of sounds, and 'right' and 'left' instead of first and second.

The preliminary practice of the observers with the first instruction lasted, excluding the summer months, as follows: F, May to November, 1915; Bo, May to October, 1915; Bi, April to October, 1915; R, April to November, 1915; E, October 20 to 30, 1915.

Thus all the observers but E had, at the beginning of the final series, fairly extended practice in adjustment to the general experimental conditions, but as yet no specific adjustment to the new instructions. We have based the discussion of this paper entirely upon the series taken under the final instructions.

Apparatus and Method

We worked throughout with Fechner's sound-pendulum[4] for judging the intensity of sounds and with Wundt's frame[5] for judging visual extents.

A new attachment to the pendulum allowed us to work with close divisions on the scale. The attachment was a metal piece, about 5 cm. long, which fitted over the scale itself; in it were scale-divisions cut through the metal as grooves. The experimenter held the pendulum by a thin piece of hard wood which he placed in a groove; he released the ball by withdrawing this piece of wood.

The observer sat with his back to the apparatus. The time-interval between the two successive sounds was one second, controlled, together with the times of the warning signals, by a soundless metronome.

On the frame were two black lines, 2 mm. in breadth. The standard (left) was 33 mm. long. The variable (right) was drawn partly on the paper beneath the glass and partly on the inner surface of the glass. The two sections slid perfectly, the one beneath the other, so that qualitative differences along the length of the line were not distinguishable.

The observer sat a distance of about one meter in front of the frame, with his head in a head-rest. A cardboard shutter on a lever was used to expose the lines. The exposure time was one second. This time and the time of the warning signals were controlled by the soundless metronome.

The judgment-times of the observers were recorded upon a sinking kymograph in an adjoining room. The kymograph was started and stopped electrically from a switch under the experimenter's control. The time-line was written by a fork of 50 vs. The pendulum and the exposure-shutter of the frame were electrically connected to mark on the drum the termination of the period of stimulation; a lip-key in the observer's mouth gave the record of the time of judgment.

In both the auditory and the visual series we worked with very small units, one scale division on the pendulum and ⅜ mm. on the frame. Usually a series contained from 7 to 10 steps; thus about half of the stimulus-differences lay below the D. L. We used a modified method of limits, in which we passed always from either 'greater' or 'less' to objective equality, irrespective of the variation of O's categories. In about half of the series a report was taken after each observation; in the other series no reports were taken.

Under our instruction, the observer knew that the stimuli would be either greater (less, in the alternate case) or equal. We supposed that such a procedure with knowledge would eliminate expectation; but it did not. Whenever the observer seemed to suspect that the method was a method of limits, and consequently to anticipate that the series would begin with a difference and end with equality, we introduced an irregular series in which the stimulus pairs were given in haphazard order.

The Observers

Our observers were Dr. W. S. Foster (F), instructor in psychology, highly practised; Dr. E. G. Boring (Bo), instructor in psychology,

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[4] G. T. Fechner, *Elemente der Psychophysik*, 1860, I, 176; E. B. Titchener, *Experimental Psychology*, II, i, 81; ii, 195.

[5] Z. Radoslawow-Hadji-Denkow, *Philos. Stud.* xv, 1899, 324; Titchener, *op. cit.*, II, ii, 257.

highly practised; Mr. H. G. Bishop (Bi), assistant in psychology, well practised; Mr. G. J. Rich (R), graduate student, majoring in psychology; and Miss G. English (E), graduate student, minoring in psychology. E was the least practised observer. F, Bi, and E worked with sound stimuli, Bo and R with visual stimuli.

Statistical Treatment of Results

Table I gives an analysis of the entire experiment under the final instruction. Reaction-times were obtained for only about 75% of the total number of judgments; some series were taken without times, and sometimes the apparatus failed to work. The individual differences in reaction-time should be noted; the average time for F is more than twice as great as for Bi.

The data of Table II were computed in order to determine whether it would be necessary to treat separately the reaction-times of the series in which reports were taken and those of the series in which no reports were taken. The table shows the excess of the average time in the report-series over the average time in the no-report-series. A negative value means that the times were longer when no reports were taken. The differences shown are all very small, and there appears to be no general trend. The averages of all observers and all categories are, in the two cases, practically identical. We have, therefore, hereafter thrown together the times of the two types of series.

Table III shows the effect of practice upon the times and upon the occurrence of the different categories. The average reaction times (secs.) are computed for successive groups of 25 series each. These groups are indicated by the numbers I, II, III, and IV. In the cases where the number of series is not divisible by 25 (F, Bo, Bi, and E; see Table I for number of series in each case) the last group for each observer is an average for all the series remaining after the preceding 25 series had been taken. In general, F, Bo, and R show a practice-effect in the shortening of the times; Bi and E do not. The averages for all observers and all categories, shown at the bottom of the table, indicate that there is, in general, little change after the first 25 series.

Table IV shows the frequency of occurrence of the various categories expressed in numbers of cases.

The average times for the different observers and categories appear in Table V. In the last line of the table are given the averages of all the cases under each category (not averages of the averages of each observer), for the purpose of the com-

parison of the categories. These values do not, however, constitute a very satisfactory means of comparison, because

TABLE I

ANALYSIS OF EXPERIMENT

Observer	F	Bo	Bi	R	E	All
Number of series taken	42	78	91	100	65	376
Number of judgments taken	377	718	799	868	586	3348
Number of judgments taken with times	269	580	579	719	382	2529
Average reaction time (seconds)	1.50	0.83	0.72	1.14	1.10	1.00

TABLE II

COMPARISON OF REACTION-TIMES FOR SERIES WITH AND WITHOUT REPORTS

Figures show in seconds the excess of reaction-times for series with reports over reaction-times for series without reports. No figures are included in this table when either reaction-time involved is an average of less than ten cases.

In this and all subsequent tables the following symbols are used to designate the different categories of judgment:

g, greater
=, equal
l, less
nd, no-difference
g=, greater-or-equal
=l, equal-or-less
g.nd, greater-or-no-difference
nd=, no-difference-or-equal
nd.l, no-difference-or-less
g?, greater-doubtful
l?, less-doubtful
=?, equal-doubtful
?, doubtful (or "don't know")

Observer	g	g=	g.nd	nd	nd=	=	nd.l	=l	l
F	—.06					+.14			—.04
Bo	0					—.10			—.04
Bi	+.06	—.02				+.04		—.02	—.04
R	+.06		+.16	—.02	+.12	+.08	+.06		—.02
E	+.14	+.04				—.22			+.10

Average reaction-times of all observers and all categories:
with reports = 1.006 secs.
without reports = 1.000 secs.

TABLE III

TEMPORAL COURSE OF THE REACTION-TIMES

Large type shows in seconds the average reaction-time for each category and each observer in successive periods which consist of 25 series each. These periods are indicated by I, II, III, and IV. The last period for every observer but R includes less than 25 series (see text).

Small type shows the number of cases upon which each reaction-time is based.

Obs.			g	g?	g=	g.nd	nd	nd=	=?	=	nd. l	=l	l?	l	?
F	I	Cases	60		1		4	1		41			4	52	6
		Time	1.62		1.68		3.20	3.00		2.06			1.66	1.14	2.72
	II	Cases	41			1				14				44	
		Time	1.00			16.00				1.60				.92	
Bo	I	Cases	52	4					5	53			3	25	1
		Time	.94	.68					1.08	1.04			.76	.92	1.24
	II	Cases	73						2	87				52	1
		Time	.74						.48	.86				.72	.66
	III	Cases	57							97				44	
		Time	.76							.82				.78	
	IV	Cases	6							14				4	
		Time	.54							.74				.60	
Bi	I	Cases	21		3	1				20		5		42	6
		Time	.78		1.00	.60				.72		.82		.74	1.02
	II	Cases	38		11					60		7		61	3
		Time	.60		.66					.70		.70		.60	.78
	III	Cases	34		12					43		14		69	5
		Time	.62		.78					.74		1.30		.64	1.12
	IV	Cases	30		8					38		6		38	2
		Time	.70		.86					.92		.92		.58	.64
R	I	Cases	20			8	31	10		23	12			36	1
		Time	1.40			2.02	1.44	1.68		1.28	1.40			1.20	1.00
	II	Cases	27	1		12	42	13		46	13			31	
		Time	.98	1.44		1.34	1.32	1.64		1.18	1.28			.98	
	III	Cases	39			18	47	8		24	8	2		40	
		Time	1.00			1.14	1.26	1.20		1.00	1.04	.96		.88	
	IV	Cases	42			17	64	8		20	16			41	
		Time	.88			1.12	1.06	1.18		1.00	1.04			.84	
E	I	Cases	59		14					24		8		20	
		Time	1.02		1.20					1.06		1.36		1.10	
	II	Cases	50		21					48		9	1	11	1
		Time	.98		1.36					1.18		1.44	1.20	1.10	1.52
	III	Cases	64		62					54		75		11	
		Time	1.28		1.24					1.08		1.50		1.00	

	Period	No. cases	Av. time
All observers and all categories...	I	676	1.23
	II	821	.94
	III	674	.87
	IV	354	.90

TABLE IV

Frequency of Categories

Figures show the number of cases in which each category was used by each observer, and total distribution of the categories in numbers and per cents.

	g	g?	g=	g.nd	nd	nd=	=?	=	nd. l	=l	l?	l	?
F	145		2	2	5	1		82	1	1	5	126	7
Bo	207	4					12	317			7	169	2
Bi	171	1	43	2	1		2	223		49	1	284	22
R	179		2	66	219	45		132	53	2		169	1
E	207	1	74				1	203		33	2	61	2
Total	909	6	121	70	225	46	15	957	54	85	15	809	34
%	27.2	0.2	3.6	2.1	6.8	1.4	0.4	28.6	1.6	2.5	0.4	24.2	1.0

TABLE V

Average Reaction-times for the Different Categories of Judgment

Figures show average reaction-time for each category and each observer, its M.V., and the number of cases upon which it is based. The general average at the bottom of the table is an average of all cases under each category, irrespective of the observer who gave the category.

Obs.		g	g?	g=	g.nd	nd	nd=	=?	=	nd. l	=l	l?	l	?
F	Cases	101		1	1	4	1		55			4	96	6
	Time	1.38		1.68	16.00	3.20	3.00		1.94			1.66	1.04	2.72
	M. V.	.59		0	0	1.09	0		.62			.14	.40	.52
Bo	Cases	188	4					7	251			3	125	2
	Time	.80	.68					.90	.88			.76	.78	.94
	M. V.	.19	.21					.30	.18			.17	.16	.28
Bi	Cases	124		34	1				161		33		210	16
	Time	.66		.78	.60				.78		.78		.68	.96
	M. V.	.16		.18	0				.19		.15		.18	.35
R	Cases	128			55	184	39		113	49	2		148	1
	Time	1.02			1.30	1.26	1.46		1.12	1.20	.96		.96	1.00
	M. V.	.23			.33	.28	.30		.21	.25	.18		.22	0
E	Cases	156	1	45					113		23	1	42	1
	Time	1.00	1.44	1.28					1.12		1.28	1.20	1.08	1.52
	M. V.	.22	0	.13					.22		.19	0	.26	0
All	Cases	697	5	80	57	188	40	7	693	49	56	8	621	26
	Time	.94	.84	1.08	1.44	1.30	1.50	.90	1.02	1.20	1.02	1.28	.84	1.40

TABLE VI

ORDER OF CATEGORIES WITH RESPECT TO REACTION-TIMES

Table shows in seconds the average reaction-times, computed irrespective of the observers and corrected for their individual differences in time (see text); also the number of cases upon which each average is based.

Category of judgment	Number of cases	Average Reaction-time
l	621	.878
g?	5	.918
g	697	.922
l?	8	1.037
nd.l	49	1.049
=?	7	1.080
=	693	1.098
nd	188	1.123
g=	80	1.125
=l	56	1.143
nd=	40	1.297
g.nd	57	1.300
?	26	1.410

the different observers furnish relatively different numbers of cases in different categories. Since the observers exhibited great general differences in time of response (Table I), the averages depend to some extent upon the number of instances furnished by a given observer under a given category. The difficulty is overcome in Table VI, where corrected average reaction-times are shown for the categories. The correction was made by multiplying each time by the ratio of the average time of the observer in question to the average time of all observers for all categories (*viz.*, 1.00 sec.). *E. g.*, since F's av. time is 1.50 sec., his times were corrected by multiplying each by the ratio, $\frac{1.00}{1.50}$ (see Table I for the times). Then these corrected times were averaged and arranged in the rank order of Table VI.

The Qualitative Study of the Categories of Judgment

In the discussion which follows each quotation from the reports of the observers is prefaced (*a*) by the symbol designating the observer, (*b*) by a number which stands for the number of the series under the final instruction from which the quotation is taken, and usually (*c*) by a word (*e. g.*, 'greater') which indicates the judgment. When the observer's remarks are general, (*c*) is sometimes omitted.

General Adjustment of the Observers to the Conditions of the Experiment.—The interpretation of the instructions gave, in general, some difficulty. Especially were the observers uncertain of the meaning of the term *attitude.* Bo and R complained that they were not necessarily aware of their attitudes, and that they could therefore not be expected always to report them.

Bo, 9. "I still do not know what a reportable attitude is. I feel sure what some such attitudes are: as doubt or expectation, but I can't tell whether some of these other factors which characterize the experience are attitudinal in the sense of the experiment or not."

Bo, 63. "I can't report expectation as a shift in attitude, because I can inhibit the conscious part of it and voluntarily take up a practically neutral set; but whenever I give 'equal' under such conditions I wonder if there wasn't some unconscious set or attitude which determined it."

Bo, 68. "I can't report this suspected expectation as a shift of attitude simply because it isn't generally conscious. I suspect it merely because I am often surprised when I get *equal* at the beginning and 'greater' at the end of a series."

R, 74. "There was a feeling of uncertainty, which has not been previously present. There was no change of attitude. I don't know what a change of attitude is, or that I ever experienced one in this whole experiment."

Bi showed particular difficulty in adapting himself to the experimental conditions. He was troubled throughout by the instructions and easily disturbed by chance distractions.

Bi, 18. "I can't make out the meaning of your *receptive attitude kept constant.* If *attitude* means my intention, the thing I try to do, then my receptive attitude is constant. If it means more than that, such a thing as my intention in addition to my attentive set, then it is not constant. I can't keep myself the same; distractions are always different. If it means that a graded series of external conditions is to be paralleled by a graded series of bodily, subjective attitudes, then I don't know whether my attitude is constant or not; I think it is not."

F and, to a less extent, E found difficulty in making the required abstraction to the attribute under consideration (intensity, since they judged the sounds). F, as compared with the other observers, tended toward a reflective and less immediate type of judgment, an attitude which might easily facili-

tate a focusing upon other attributes of the stimulus. Undoubtedly the mediate judgment occurs more readily when the presentation is successive (*i. e.* with sounds; F, Bi, and E) than when it is simultaneous (*i. e.* with the lines; Bo and R). In the former case the observer frequently tries to retain the first member either in imagery or in some other surrogate conscious process until the second member appears; or else the second member is consciously anticipated. Of the three observers who worked with successive presentation, the two giving the longer times (F, 1.50 sec. on the average; E, 1.10 sec.) had difficulty in making the abstraction to intensity. The average times for the two modes of presentation (averages of all cases, not of observers) are practically identical (1.008 and 1.001 sec.), because Bi gave immediate judgments with successive presentation. (Bi's av. time, 0.72 sec., was shortest of all.)

F, 11. "There *was* a difference between the two sounds. I hesitated; I wasn't sure whether the difference was one of intensity or not; made up my mind that there wasn't any difference and said *equal* without, therefore, great assurance. I was struck, not with the *equal* of them in intensity, but with their difference as sounds. Perhaps I should have said *no-difference,* but since I did decide that their difference wasn't a difference in intensity and since I was set to judge intensity, I naturally said *equal.*"

F, 17. "I keep saying *intensity* to myself. In spite of these repeated instructions to myself, the sound as a whole very often catches my attention, so that the first impression is sometimes different, and I have afterward to make up my mind about the intensity."

Many similar passages occur in F's reports up to the end of the experiment.

E, 20. "They seem so different that I couldn't seem to compare them. I suppose I mean by that that their differences impressed me so that I couldn't catch the difference in intensity if there was any. They are qualitatively different in some way that I can't describe." . . . "Seemed as if I were reading something else into the experience besides intensity; I don't think it was tone, but I'm not sure that it wasn't vaguely that. The second seemed to go up."

Doubtful Judgments.—In 34 cases (1% of the total number) the observers reported *doubtful* or *don't know.* Two-thirds of these judgments were given by Bi (22 cases: Table IV), who experienced especial difficulty in adapting himself to the experimental conditions. The judgment was usually recognized as a failure of the instruction. The most frequent cause assigned was a 'lapse of attention.' It is obvious that a state of absolute doubt reflects a shift in the psychophysical conditions which violates the assumptions of the method; such judgments must either be ruled out or discarded, as, indeed, it is customary to do.

In some cases the observers pointed out that an absolute doubtfulness represents a change of attitude:

Bo, 31. "That is a miscarriage completely. Everything was normal up to the time that I took my lips off the key; then I discovered to my surprise that I had no judgment ready. I had to look back, was very much disturbed, couldn't make up my mind, and said *doubtful.* The course at first seemed normal, but I think that the failure of the judgment to come out automatically must have meant that my attitude had not been normal, that it had changed unconsciously."

Bi, 16. "I was wondering about the standard when the variable came; that is why I said *doubtful.* I didn't know much about the variable. This is a different receptive attitude, an active one. The attitude I am trying to keep constant is receptive passivity."

In 36 cases (again about 1%: Table IV) the observers gave other categories of judgments *as* doubtful, that is to say, they reported *greater doubtful, equal doubtful,* or *less doubtful.* In a very great many more cases their reports show that they were doubtful, although the judgment was not given as doubtful. It is not possible, however, to treat this second class of doubtfuls statistically, because no sharp line of division can be established. The judgments were given as 'doubtful,' 'probably doubtful,' possibly doubtful,' 'hesitant but probably not doubtful,' 'delayed and therefore doubtful,' 'delayed but not doubtful,' and so on. The relation of hesitancy to doubt we shall take up later.

Bo, 1, greater-doubtful. "Started to say *greater,* at least I know my vocal-motor apparatus started to say something, and I took it to be *greater.* Somehow that incipient utterance did not seem to follow from presentation of lines. I looked at left line, then right again; saw right as longer. Moment of hesitation; then I said *greater* (possibly my vocal-motor apparatus was going to say something anyhow) and added doubtful as a sort of compromise."

Bo, 2, less-doubtful. "First pair seen together as equal. Then right line got less, without getting more distinct; then it seemed to change to an extent between less and equal. I was all set by that time to say less, and as the right seemed to waver slightly between less and the intermediate point, I said *less-doubtful.*"

Bo, 7, equal-doubtful. "Rapidly shifting consciousness. First the left line, then the right as equal. Then left, then right, with left as less. Then left, then right, with left at first as equal (a pair); then rapidly changing to less, and then to a doubtful length, *i. e.,* visual extent actually hazy and inclined to waver. . . . A compromise with the equal grew out of the final wavering." Equal-doubtful: "Not a fair judgment, since *equal-doubtful* means I don't know whether it is equal or greater or perhaps less, and therefore it is not a serial judgment, since it does not lie upon one side or the other of equal."

E, 51, less-I-think. "I wasn't perfectly sure of it. It simply put me into the doubtful attitude."

It is clear from the reports that the observers recognized

that these mixed doubtful judgments constituted a violation of the constant attitude required by the instructions.

Bo, 10, equal. "A little doubt after I started to say *equal.* Up to that point the course was normal" [*i. e.,* showed a constant attitude].

Bo, 60, equal. "Normal so far as my tentative judgment, but just as I started to say *equal* there was a catch in my throat and I was thrown into an attitude of doubt. Then the judgment *equal* came on out automatically."

Bi, 53, equal. "Or less. I don't quite know why I was doubtful, or why there were two probable values for the judgment. I think my attention slipped; I don't know just where this slip came."

We must note that there has as yet been no conclusive evidence of a serial position of any *doubtful* category. We can not say that doubt lies here or there in the series of impressions. That the mixed doubtfuls occupy intermediate positions between the positive categories might be argued at this juncture; we must, however, defer discussion of the point until we have considered the 'or-judgments.'

Bi, 63, equal. "Wanted to say *equal,* but that wasn't enough. It may be a *doubtful-equal,* but I don't think I know what that means. Experience largely visual. The variable left me with a gray spot in a visual field, with a dark hole besides. That hole seemed to be the *doubtful,* a blankness as at first." Bi has elsewhere tended to correlate the experience of 'blankness' with the attitude of doubt.

Bi, 85, less. "But I am doubtful. My attitude was pretty bad, particularly at the time the standard was given."

R, 11, less. "Along towards the end of the exposure, there occurred this same kinaesthetic reaction which carried the meaning *less.* But there was also a slight doubtfulness, which seemed to be an organic or kinaesthetic thing. It seemed to have inhibited or delayed the report. This doubtful feeling carries a sort of meaning or set not to report the judgment because it isn't right. This feeling disappeared quickly and then the report came." A positive case of the violation of constant psychophysical conditions by the attitude of doubt!

R, 22, less. "There was a baffled, puzzled feeling, which also seemed to be a kinaesthetic thing of the same nature as strain. It is the sort of thing that makes a doubtful judgment, although it wasn't prominent enough in this case to call the judgment a really doubtful one."

E, 5, less. "I didn't feel satisfied with this one. It might have been *equal.* I don't seem to know anything more about it. The judgment just comes. Sometimes I am satisfied with it and sometimes not."

E, 56, equal. "Both seemed *equal,* I think." Less: "I felt perfectly sure of judgment." Equal: "Didn't feel very certain about it." Equal: "Perfectly satisfactory and normal;" thus E implies that doubt is unsatisfactory and not normal. Equal: "Didn't feel as sure of that as of last." Many other passages in E's reports are similar to these.

The case against the *doubtful* judgments is substantiated by the quantitative results.

In the first place, the *doubtful* judgments are the least frequent of all the categories used (see Table IV). Beginning with the least frequent category, the rank order is as follows: greater-doubtful (0.2%), equal-doubtful (0.4%), less-doubtful (0.4%), doubtful (1.0%), and then on through the 'or-judgments' and the judgment 'no-difference' to less (24.2%), greater (27.2%), and equal (28.6%). It appears that the most infrequent categories might well be those against which the instruction to maintain a constant attitude had operated.

Furthermore, the *doubtful* judgments tend to drop out as practice progresses, as if with the firmer establishment of the constant attitude (see Table III). In the first 25 series 4.4% of the judgments with times are doubtful judgments; in the second 25 series, but 1.1%; in the third 25, 0.7%; and the last group of series, 0.6%.

The reaction-times furnish some evidence (see Table VI). The average time for the judgment *doubtful* (1.41 sec.) is longer than for any other category, and very much longer than the times for the *less* (0.88 sec.), the *greater* (0.92 sec.), and the *equal* (1.10 sec.). (It will be shown presently that the other and intermediate categories are closely related to the *doubtful* judgment.) These long reaction-times doubtless indicate that the judgment *doubtful* is of the reflective type; that it is given less immediately and as the result of a more complicated mechanism than are the positive judgments; and that it is, in so far as it breaks away from the simple correlating mechanism which the psychophysical procedure demands, inadmissible.

The mixed doubtful judgments, 'greater-doubtful,' 'less-doubtful,' and 'equal-doubtful,' do not appear to be given after longer times than their corresponding positive judgments (see Table VI). They are perhaps based upon too few cases (5, 8, and 7 respectively) to be representative. The values are, however, not unexpected. The positive judgments (greater, less, equal) include many reflective cases, many doubtful cases, and many cases in which doubt occurred immediately after the utterance of the judgment. The mixed doubtful cases include doubtful cases and some cases in which doubt appeared just as the utterance was being initiated and was added in the spoken judgment. The two classes of categories are, therefore, not so different as might at first appear. We have already noted that it was impossible to draw a sharp line of division, and to separate in the statistical treatment the really positive judgments from those for which positive categories were employed, but which were shown by the reports to have been more or less doubtful.

Extra-Serial Attitudes Other Than Doubt.—The observers tended to meet the difficult instruction with a complex response. This complexity was especially evident in the early series, before the judgments had become as nearly mechanised as they were later, and in the judgments of the more 'reflective' observers who did not under practice develop the immediate type of response. The following report of F is a case in point:

F, 6, greater. "Before the experiment I told myself to get into an attitude to keep constant, *i. e.* not to be too expectant that the second sound would be greater, but to set myself more or less as if this were an experiment in right and wrong cases. The first sound was heard clearly and remained in a kind of memory-after-image, or in a kind of steady intensity of strain which meant that I was holding on to it. Then the second sound was heard clearly as louder than the first, and immediately I said *greater*. Then relief. I was very certain of my judgment. I should say that the first sound is actively attended to. I expected it and held it after it had come. The second sound I hear more passively."

Here we have hints of many possible failures of a 'constant receptive attitude.' In the course of one judgment mention is made of expectation, relief, certainty, active attention, and passive reception. Do these states constitute attitudinal shifts which violate the instructions?

Fortunately for the experiment, the passage quoted may be regarded as an exception. All the observers, including F, tended under practice to acquire a greater constancy of attitude, which came about by way of a simplification of the mechanism of judgment. Bi, as we have already observed, alone failed to reach a sympathetic adjustment to the problem. Again and again he would be upset by distractions both within and without the experiment. The following report is typical of the more subtle form of disturbance to which he was subject:

Bi, 52, equal. "Conscious of time being longer between standard and variable. This is an observation of the duration of such strains as breathing strains, that gives me a different attitude than usual for my judgment."

Next to doubt, expectation constituted the most frequent mode of departure from what the observers styled the 'normal' attitude. Under expectation the observer expects something specific, some particular category of judgment or value of stimulus, and the thing expected naturally changes from observation to observation within a series: a change which is presupposed in the method of limits and, to a less extent, in the method of constant stimuli. Expectation becomes

therefore a particular enemy of the constant receptive attitude for the reason that it tends to be specific and to shift, not with the stimulus, but with the progress of the experiment.[6]

F, 8, equal. "My attitude that time was that I expected them to be near equal. The two came to me as equal; I went over them in imagery, still equal; so I said *equal.*" Greater: "Still expected them to be equal. Second came as a little louder."

F, 10, less (long reaction time; given after five judgments of greater). "I couldn't help expecting *greater* because there had been so many. The first sound struck me as being absolutely loud; a little surprise that the second one wasn't so loud; I hesitated, then said *less.*"

Bo, 11, less. "Same as before, except that I anticipated 'less' a little."

Bo, 35, greater. "I think that perhaps in my 'normal' attitude there is a little expectation of *greater,* and that *equal* partakes somewhat of the nature of a negative judgment, *i. e. equal* is *not-greater.* I should not say that the *equal* judgment is entirely defeated expectation, yet in one sense I think the *greater* judgment is the more expected. The thing is very subtle; it is hard to get at the expectation consciously; I should say it was generally unconscious. I just get hints of it once in a while, so that I can't be sure that it isn't there most of the time."

Bo, 45, equal. "I think I was a little disposed toward *equal* this time; I rather expected a series and that it was now about time to reach *equal.* I had misgivings about my judgment after I had made it."

Bi, 20, equal-or-greater. "After the signal I took the trouble to recall the previous pair. I think that made a difference in my judgment. It was harder for me to judge as I did because I had to get rid of this sort of expectation before I could judge the pair in its own right."

Bi, 23, equal. "Expected *equal* quite strongly. Standard was weak; that is why expectation was strong. My whole attitude seemed to shift when I found I was wrong on the standard."

Bi, 61, less. "I expected this less than it turned out to be. After I had said *less,* I felt quite positive that I might have said *equal* if I had been expecting *equal.*"

Bi, 73, less-or-equal. "It may be I expected an *equal,* but I really found it pretty positively less. After that I returned to *equal* as I reported it. I can't tell whether I said *equal* through the persistence of expectation, or whether the stimuli actually warranted *equal.*"

R, 75, greater. "I think there was a change in expectancy there, that is to say, you have been going along with serial judgments and I expected greater-or-no-difference in that region. I think I was set for such a judgment." "What I understand by attitude hasn't changed at all in the experiment, so far as I am aware. I have always put my judgments in the same serial categories, the attitude toward that has been constant. There have been, of course, changes in expectation, which I did not understand to be meant; so I didn't report them." R has still not thoroughly understood the instructions; his

[6] There can, of course, be constant expectations, which need not and can not be avoided. Such, for example, is the general expectation for 'equal or greater' or 'equal or less,' as the case may be, which was determined by the instruction for the two sorts of series.

report above indicates clearly enough that expectation meant a change of disposition toward the stimuli.

E, 16, equal-or-greater. "After the first sound a feeling that it was going to be all right now, that the next was going to be definitely less. When the second came it surprised me that it was as loud as it was." E gives many other reports of surprise which imply an expectancy. She does not explicitly characterize the attitude as a violation of the condition of constancy.

The general complaint of the observers, which we have pointed out above, namely, that attitudes may be unconscious and as such unreportable, is made specifically against expectation. The second quotation from Bo is relevant, as are also many from the reports of Bi:

Bi, 18, equal. "I intended to judge that one as something less than *equal,* though I didn't realize this intention until I had given the judgment. This amounts to an unconscious set suddenly becoming conscious, as if the previous judgment gave me an attitude for another like it. This attitude was unconscious, but when the stimuli seemed to demand the judgment *equal,* I seemed doubtful that it was *equal,* purely because I seemed to expect it to be like the preceding judgment. That is a shift of attitude, then. I don't know whether I have had expectations for other judgments or not, but I suppose I had. If I had they were unconscious."

Bi, 43, equal. "I expected that to be *greater.* It is again a case where the expectation does not become conscious until the variable is given."

Bi, 51, equal. "The feeling that the variable might be less even. Certainly I wasn't expecting *equal.* The *less* may have come from an unconsciously expected *greater.*" And many other similar passages.

Bi alone of all the observers suggests that expectation may sometimes not vitiate the required constant attitude. We have had plenty of evidence from Bi as well as from other observers that it also frequently does, enough evidence, in fact, to make us insist upon its exclusion.

Bi, 23, equal-or-less. "I certainly expected *greater,* but not until after the standard had come. I was indifferent up to that time. No struggle at all, however, as to which judgment was right; the expectation wasn't insistent. It didn't seem to modify the feeling I had toward the judgment any more than if it were breathing."

Bi, 63, less. "I expected *greater.* I didn't seem at all surprised that the variable should come as less, and I didn't notice that expectation modified my judgment at all. The expected variable seemed irrelevant to the judgment."

These two cases are the only two explicit mentions of the irrelevancy of expectancy to the problem made by any observer.

The serial mode of presentation of the stimulus pairs (modified method of limits) favored the occurrence of expectation, in spite of the attempt to keep the observers in ignorance of the method by the occasional interjection of irregular orders

of presentation. All the observers but E explicitly mention a shifting expectation that grew out of a suspicion that a method of limits was being followed. For example:

Bo, 35, greater. "A *greater,* I think, was expected. At least at the beginning of a series, which I expect in a somewhat serial order, I am inclined to expect *greater.* I am not sure that I do not therefore shift my attitude as the series continues by expecting *equal* more and *greater* less. The thing is very subtle; it is hard to get at consciously;" *etc.*

Bi, 59, equal (the second judgment in a series which began with *greater*). "Or less. I think I'll say 'or less' because I expected *greater* as I do at the beginning of a series."

On the basis of such observations it would be in order to raise the whole question of the propriety of the method of limits, which puts a premium upon a shifting expectation. In the method of constant stimuli the rule that the observer should be told when each new series is begun is also open to question. If the subject is to be kept constantly disposed for the whole series under investigation, it is surely not permissible to allow dispositional shifts within that series.

It is next in order for us to enquire what other attitudinal changes besides doubt and expectation are likely to interfere with the maintenance of a constant disposition.

Surprise is mentioned frequently, but it seems in most cases to reduce to a form, or at least a concomitant, of expectation.

Bi, 31, greater-or-equal. "I wasn't conscious of any expectation, yet it does look as if I expected it to be *greater.* The same thing has happened a number of times: I feel surprise after the judgment. It seems as if this surprise comes as an unconscious expectation or valuation of the variable."

E, 16, equal-or-greater. "The first sound surprised me by being so soft; and then the second when it came surprised me too. I had the notion it would be very soft, and it surprised me by being so loud."

Surprise may, however, appear to leave the judgment unaffected.

Bo, 23, greater. "My attitude, besides being 'normal,' was one of surprise, although the surprise seems somehow irrelevant to the judgment."

A report of a positive attitude of *certainty* is exceptional. The 'normal' attitude seems to have been passive, neither doubtful nor certain, although it is possible logically to interpret the immediacy of the 'normal' judgment as a form of unconscious certainty. In several of the few cases in which certainty was reported as an attitudinal change, it was reduced directly to surprise and indirectly to expectation:

Bo, 12, greater. "There was the degree of expectation which a conscious reference to the instructions has. I knew that I was under

a different *Aufgabe* from the 'less-or-equal' instruction. I was shocked by the line, *i. e.*, shock = surprise at bigness of line = certainty of judgment. I should say that this certainty, due to surprise, was a definite shift of attitude."

There are no other types of attitudinal shift which are reported with sufficient frequency and positiveness to merit separate discussion. All the observers suspected dispositional variations at times, but their suspicions were not confirmed. There are a good many isolated statements like the following:

Bo, 12, greater. "For the most part a mechanical judgment, but just at the end of the period, before the judgment, I was definitely struck by the bigness of the line. I suppose that is an attitudinal shift." (This judgment was less immediate and more 'reflective' than Bo's 'normal' judgment; hence we must return later to the case when we discuss the immediate judgment as the normal judgment.)

The 'Or-Judgments.'—The judgments 'greater-or-equal,' 'equal-or-less,' 'greater-or-no-difference,' 'no-difference-or-equal,' and 'no-difference-or-less' were given in 11.2% of all cases (see Table IV for the distribution). Bo gave no such judgments and F but seven, so that for the description of the 'or-judgment' we must look to the other three observers. Of these Bi gave 94 and E 107, which were practically all either 'greater-or-equal' or 'equal-or-less.' R used the category 'no-difference,' and practically all of his 168 'or-judgments' were the combinations of 'greater,' 'equal,' or 'less' with 'no-difference.'

The descriptions of the 'or-judgments' are of three kinds. (1) The 'or-judgment' may reflect a mixed basis of judgment or a conflict between bases; (2) it may be the outcome of an attitude of doubt; or (3) it may appear to constitute a serial category in its own right. The first case shades imperceptibly into the second, since conflict is apt to beget doubt. Of the validity of the third case we shall have presently to enquire.

Both Bi and R report many cases in which an 'or-judgment' results from a twofold basis of judgment or from a vacillation between two conditions of judgment.

Bi, 27, equal-or-greater. "When variable came I had two spots of light, about same size, corresponding to intensity. The sizes were the same. I think in terms of throat kinaesthesis and I am inclined to believe that the difference came in there. It was somehow as if visually they were equal and kinaesthetically they were different."

Bi, 41, equal-or-greater. "Don't know why, except that there seemed to be two values for the variable in relation to the standard."

Bi, 42, equal-or-less. "Judgments of that sort have double value, the values of the variable compared with two different probable values of the standard. I don't know where the two values come

from. Such judgments are carried visually sometimes. The value of the variable is a point; the value of the standard is a line or an area and sometimes approaches the dumb-bell of cutaneous perception; the two ends are the two values of the standard."

Bi, 69, equal-or-greater. "Thick and short vertical visual imagery, which stood for both. I don't know why I changed to *greater*. A case of succession. Apparently I had a spot higher up which stood for the *greater*, while the line stood for both *equal*."

R, 2, no-difference-or-greater. "In that case there seemed to be a vacillation of judgment. At first the two seemed equal, although I do not know in what terms this equality came to consciousness. Then came the idea that the right was greater. The new judgment seemed to be principally conditioned by a kinaesthetic reaction to the stimulus which carried over in imagery from the end of the stimulation. I don't think there was any visual image of the lines to condition this change. The change came so late that I reported *no-difference*. As near as I can tell the doubtfulness of the judgment consisted first in the fluctuations of the judgment and secondly in the absence of a feeling of sureness."

The equivalence to doubt of a fluctuation of alternate bases of judgment is still further emphasized in the following instances:

R, 4, no-difference-or-equal. "That was a distinctly doubtful judgment. The two ideas alternated, *i. e.* the two meanings carried in kinaesthetic terms."

R, 14, greater-or-no-difference. After describing two meanings, kinaesthetically carried, for the two terms of the judgment, R continues: "I think the two meanings alternated, but I am not sure of it. Over the whole judgment-consciousness there was a general feeling of uncertainty."

E, 51, equal-or-greater. "Very, very doubtful. I seemed to hesitate, to go from one judgment to the other. I couldn't make up my mind."

Occasionally expectation conditions an 'or-judgment;' there is conflict between an actual impression and an anticipated impression:

Bi, 37, equal-or-less. "I think *equal* was what I expected and *less* was what the variable seemed really to call out. It is the same kind of unconscious expectation that I mentioned before. What I did was to turn from what presumably would have been a *less* without the expectation into the class of *equal-or-less*."

Bi, 59, equal-or-less. "Not very positive of expectation for *greater*. It may be that the expectation gave the 'or-judgment.' The standard was completely lost in the expected variable, so that I judged the variable against a standard transformed into the expected variable."

E, 34, less-or-equal. "The second one startled me, it seemed so small. When I started to speak, however, the word *equal* was in my mind, so that I said *less-or-equal*. The thing really struggling to be said was *less*, but there seemed some doubt about it and I said *equal*."

Equivocally determined judgments like the foregoing are frequent; they do not, however, form the greater part of the

'or-judgments.' The great majority of the 'or-judgments' are definitely doubtful judgments.

F, 8, equal-or-no-difference. "Quite open-minded. The two sounds came to me as about equal; I went over them in imagery trying to see if I could tell what the difference was if there was any difference. Still felt doubtful whether there was any difference, so I said *equal-or-no-difference.*"

F, 27, greater-or-no-difference. "I wanted to say *greater-or-equal* instead of *greater-or-no-difference.* When the sound came I was immediately doubtful, as the second sound did not come as either different or not different; then it seemed to me a little greater; then it seemed equal; then I wanted to say *greater-or-equal.* Then I thought of my instructions [where 'no-difference' is suggested as a possible serial category] and it seemed to me as if *equal-or-greater* might not be in a serial line. Then it struck me that *greater-or-no-difference* satisfied the conditions pretty well, because, although the second sound did not come to me as positively not-different, neither did it come as positively different. I really think that *equal-or-greater* is my best judgment, for I think that it is serial." (An excellent example of the reflective type of judgment, so common with F!)

Bi, 18, less-or-equal. "I was awfully puzzled about that one. And while I was puzzled and waiting for some cue to come, everything looked intensely black. I don't think the blackness was important except that it was mixed with the hesitation."

Bi, 42, equal. "To say that that is *equal* is not saying enough. It is a *doubtful equal,* that is to say, what I call *equal-or-less.* Judgments of that sort have double values;" *etc.*

R, 8, less-or-no-difference. "The chief characteristic of that judgment was an organic feeling which meant uncertainty. I don't know why I knew the judgment was less."

R, 14, equal-or-no-difference: ". . . Also the feeling of uncertainty." Followed by *equal:* "This was different from the last one only in the absence of the feeling of uncertainty."

R, 26, less-or-no-difference. "The chief difference between this judgment and the last one [which was *less*] is the feeling of hesitancy which came in and prevented my reporting the feeling *less* which I had first. This hesitancy is a kinaesthetic thing and seems to be a sort of catching of my breath. The judgment practically amounts to a doubtful judgment of *less,* which, in order to express it in these terms [suggestion of the instruction?], makes me say *less-or-no-difference.*"

R, 40, equal-or-no-difference. "That was a very doubtful judgment. The whole judgment-consciousness was colored by a feeling of indecision, hesitancy, or doubtfulness."

R, 52, equal-or-no-difference. "This was a doubtful judgment. It seems to differ from the last one [a *less*] in so far as the meaning of the judgment seems to be held up and not to come. This holding-up or hesitancy seems to constitute the doubt and is the only difference between it and the straight judgment *less.*"

R, 77, less-or-no-difference. "The *no-difference* judgment really means doubt, which seems to be represented by the slowness of any judgment coming. I had the sensations from the two lines, before they meant either *equal* or *less.* As a result I gave the doubtful judgment. The judgment at first didn't mean anything; finally it did come to mean lessness a bit; the *no-difference* part is the *doubtful.*"

And so on in almost all of the later instances. R's identification of the 'or-judgment' with the doubtful judgment is practically absolute.

E, 1, greater. "I don't feel perfectly sure of that judgment; I really ought to have said *greater-or-equal.* It seems as if it were hard to find the difference."

E, 10, equal-or-greater. "Doubtful feeling about that."

E, 14, greater-or-equal. "Doubtful. . . . . A feeling of dissatisfaction in not being able to find a clear cut difference."

E, 16, equal-or-greater: After describing a dual basis of judgment, E concludes: "It seemed to me first *greater,* then not; but there was right away an indecision about it. I simply couldn't feel sure whether it was equal or greater." Equal-or-greater: ". . . . It was a doubtful judgment; I wasn't sure of it at all."

E, 28, equal-or-greater. "There was no sureness about that at all. I think my impulse was to say *greater,* and immediately on top of that came *equal; equal* popped out, but still I wasn't satisfied with it and so added *greater."*

E, 51, equal-or-greater. "Very, very doubtful. I seemed to hesitate, to go from one to the other: I couldn't make up my mind."

There are many similar passages in E's reports, all of them indicating that the 'or-judgment' is a doubtful judgment. E does not explicitly identify the two, but seems to take the identity for granted. By implication she regards her 'or-judgments' as failures.

There is now little room for doubt that the 'or-judgment' indicates essentially an infringement upon the constant receptive attitude. Instead of being the expression of a particular impression, as the psychophysical procedure demands, it is a logical compromise between two incompatible impressions. The very existence of two rival impressions in the focus of attention implies a shift of attitude, which is definitely recognized in those cases where the alternate impression is the result of expectation. Usually psychophysical constancy is still further disturbed by the shift to an attitude of doubt. 'Or-judgments' of this character are plainly not allowable in psychophysical work.

But is there not, one may ask, a definite, positive, intermediate, serial impression which is neither, let us say, 'equal' nor 'greater,' and which corresponds to a judgment of 'equal-or-greater?' Bo, who gives no 'or-judgments,' and F, who gives but few, do not find it. R explicitly denies it, and E implicitly. Bi alone hints of the existence of such a category, and that in only three places:

Bi, 49, less or equal: "That seemed normal enough. That 'or' wasn't due to expectation. A case of very near *equal;* as soon as I'd said *less, equal* just followed and seemed just as good. It is just like that. I don't know how the *equal* followed upon the *less."* Equal-or-greater: "This was just like the one before, except that the judgment was different. It comes spontaneously, as far as I can see."

Bi, 59, equal-or-greater. "That was a case of good conditions, but the difference was so small that I couldn't make it out."

These, however, are but three cases in three hundred and seventy-six. Elsewhere Bi agrees with the other observers. It therefore seems that, even if there is a definite serial impression corresponding to the 'or-judgment,' it is not indispensable; whereas the undiscriminating admission of the 'or-categories' in a psychophysical group is almost certain to destroy the serial nature of the group.

Finally we must add the statistical evidence. The 'or-judgments' do not tend to drop out under practice (Table III), but they are avoided under the instruction by the two most experienced observers, F and Bo. Consequently they constitute, after the doubtful judgments, the smallest categorical groups (Table IV). (The order, after the doubtful judgments, beginning with the least, is: 'no-difference-or-equal,' 'no-difference-or-less,' 'greater-or-no-difference,' 'equal-or-less,' 'greater-or-equal.') All five categories together form a group which is less than half the size of the group for 'less' or 'greater' or 'equal.' The infrequency of the 'or-judgment' argues that it may be dispensed with.

In general, the reaction-times for the 'or-judgments' are intermediate between the times for 'doubtful' and the times for the positive judgments, 'greater,' 'equal' and 'less.' Four of the five 'or-judgments' lie in the order of categories immediately below 'doubtful' (see Table VI). The average reaction-time of the five 'or-categories' is 1.17 sec.; of the three positive categories, .96 sec. It appears, then, as if the ambiguous and doubtful nature of the 'or-judgments' resulted in the lengthening of their reaction-times.

The Judgment 'No-Difference.'—The category 'no-difference' was employed almost exclusively by R (225 times). F used it five times, Bi once, Bo and E not at all.

As with the 'or-judgments,' so here we can find some slight evidence that 'no-difference' is a positive category.

R, 33, no-difference: "This was more like the *less* judgments. No particular doubtfulness on it; simply a different kinaesthesis that means a *no-difference* judgment." Later, *equal* following a *no-difference:* "The condition of change from the last judgment seems to be the presence of what I might describe as a positive feeling of equality; it is kinaesthetic and different from the general feel of the *no-difference* judgment. On the side of meaning it is quite different, for *equal* means that the two extents are actually the same, while *no-difference* simply means that they are so nearly the same that I can not tell which way the difference lies, if there is one."

R, 50, no-difference (following many series in which *no-difference* has been equated to *doubt*): "The judgment-category *no-difference* seems to be taking on a meaning of its own, apart from the doubtfulness which has been associated with it. The whole consciousness is simplifying. Instead of a *no-difference* judgment being indecision

between *greater* and *equal,* as it was in the preceding case, for instance, it is now as definite a judgment as *greater* or *equal* would be." But R never repeats this observation, and in Series 53 and repeatedly thereafter we find him again identifying *no-difference* with *doubtful.*

F, 2, greater. "I don't think I meant to say *greater* at all. . . . I really should have said *not different.* They were not different; not that there was no difference. When I say that there is definitely *no difference* I mean that there has been a true comparison, a true comparative attitude. *No-difference* seems positive. This was scarcely a real judgment, scarcely a comparison." This in Series 2; but F finds occasion to judge *no-difference* only five times in 377, and ceases entirely to use the category after Series 16.

Like F, Bi hints at a *no-difference* impression without using the category.

Bi, 70, equal. "Was distracted. Think I said *equal* because I couldn't find any difference,—a quite different experience from saying *equal* because they are equal."

Bi, 73, equal. "I said *equal* because I couldn't find any difference. I had a feeling that it shouldn't be *equal,* that expectation gave me the feeling to say *equal* because I couldn't find any difference."

We could make out a case against the 'no-difference' judgment on the basis of these positive instances alone. The category is used extensively by only one observer (R) among five; the other observers who use it (F, Bi) do so only in the early series; its identification as a positive category is exceptional, for no observer maintains this opinion for any length of time; R gives the most definite evidence (see above) that 'no-difference' is a positive category, but he also gives the most evidence (see below) that it usually is not; in the first two of R's three instances (see above) he admits that he was judging kinaesthetic surrogates and not the visual extents; in R's second instance above 'no-difference' appears to be a broad category lying neither to the one side nor to the other of 'equal,' and as such it is extra-serial and consequently taboo under the instructions; and finally R (second quotation) and Bi (both quotations) imply that 'no-difference' is really after all, not a positive, but a negative judgment, that it represents a failure to find either a positive difference or a positive equality, and thus, by implication, that it stands for a dispositional shift from a positive receptive attitude to a negative or defeated receptive attitude.

We have decided against the 'no-difference' category because of the insufficiency of its credentials. Now let us see what the direct evidence against it is, that is to say, how definitely it can be identified with doubt. For this category R is, as we have said, the important observer.

R, 2, no-difference. "The judgment showed no oscillation from one category to another as did the last one [an 'or-judgment'], but it was by no means sure."

R, 4, no-difference. "There was a feeling of doubt which made me say *no-difference* instead of *equal.*"

R, 8, no-difference. "The first thing that I experienced toward the end of the exposure was a feeling of uncertainty. I was aware that I had no feeling of *greater* or *less,* and gave the report."

R, 11, no-difference. "The total consciousness there seems to be one of questioning or doubt, that is to say, I can find no difference between the two extents and this fits logically with the *no-difference* judgment."

R, 22, no-difference. "Here the prominent thing in the judgment is this feeling of 'puzzled' or 'baffled,' for the judgment *no-difference* as I gave it only amounts to a doubtful judgment. It seems due to a lack of prompt reaction which means a definite judgment such as *equal* or *less.*"

R, 26, no-difference. "This was really a *doubtful equal* judgment. The general kinaesthetic set which means *equal* was present, but there was also a feeding of hesitancy."

R, 27, no-difference. "This was a typically *doubtful* judgment, *i. e.,* the predominant thing in consciousness was the hesitancy. The kinaesthesis which carried the meaning of a definite judgment seems to have dropped out. The feeling of hesitancy seems to be coming to mean a judgment of *no-difference. No-difference* is a report for the typically uncertain judgment or doubtful judgment. In the case of a *no-difference* with another judgment, as 'no-difference-or-less,' the *no-difference* comes to mean that I am not confident of the other judgment."

This kind of report is made again and again with too little variation to warrant frequent quotation. We have already noted that R departed from the interpretation of 'no-difference' as hesitation or doubt in Series 33 and 50. After each departure, however, he comes back to the old description. A few more samples, selected at wide intervals, will suffice.

R, 50, no-difference, given just before the 'no-difference' judgment quoted above in which R asserted the positive character of the 'no-difference' category, and just after a 'greater' judgment: "This differs from the preceding in the presence of a feeling of uneasiness, which means uncertainty in the judgment."

R, 53, no-difference. "This judgment is of the general character of doubt. There was kinaesthesis present which meant doubt. The preceding judgment [a *less*], unlike this one, seemed to come automatically." Equal, after a 'no-difference': "The general kinaesthetic feeling which carried a meaning of doubt or uncertainty disappeared."

R, 69, no-difference. "This is a typical uncertain judgment. A kinaesthetic feeling carried the meaning of uncertainty, or, as it is now coming to be, the meaning of *no-difference.* This kinaesthesis means *no-difference* directly, though I can see that it means doubtfulness also."

R, 77, no-difference. "Here even more typically doubtful judgment than the last [an 'or-judgment'], though the uncertainty was a kinaesthetic thing, a sort of tingling kinaesthesis in the chest which means uncertainty. For such a thing I use the category *no-difference.*"

R, 91, no-difference. "Now this kinaesthesis, which carries the meaning of uncertainty, has become the most prominent thing in consciousness. It now seems to carry the whole meaning of the judgment, whereas before it just seemed to come in as something additional."

We can also quote Bi on the equivalence of 'no-difference' to doubt.

Bi, 15, greater-or-no-difference. "First the stimulus meant *greater;* then came a feeling of doubt and the thing meant *no-difference.* The whole experience was colored by doubt." No-difference: "The experience in the *no-difference* judgment is essentially an experience of doubt. Considered logically the stimuli are neither equal nor different. Kinaesthesis carried the meaning of uncertainty." We may guess that Bi recognized here that the judgment 'no-difference' violated the instructions, and that for this reason he never gave it again.

F was inclined in the early series, where he used the 'no-difference' category, to equate it to a particular kind of doubt.

F, 8, equal-or-no-difference. ". . . *No-difference* means no difference in intensity here. I wanted to say *doubtful,* and stopped myself from saying it because I wasn't entirely doubtful. *Doubtful* would have meant, 'I don't know anything about it.'"

F. 17. "I very often find trouble and sometimes have to give a *no-difference* judgment."

In the face of all this evidence it is impossible that we should admit the category of 'no-difference' in psychophysical procedure any more than we admit the doubtful categories. Four of the observers by their avoidance of 'no-difference' showed that the instruction tended to exclude the term, even in spite of the fact that it was explicitly suggested in the instruction. With R the suggestion of the use of the term in the instruction worked, although in working it did violence to the operation of the major purpose of the instruction (the maintenance of a constant, receptive attitude), as his reports abundantly show.

The reaction-times for the 'no-difference' judgments are on the average intermediate between the times for the 'or-judgments' and the times for the positive judgments and nearer the former, suggesting that 'no-difference' is a slightly more immediate kind of doubt than are the 'or-judgments.' (See Table VI. The av. times are: Positive judgments, 0.96 sec.; 'no-difference,' 1.12 sec.; 'or-judgments,' 1.17 sec.).

The Judgments 'Greater,' 'Equal,' and 'Less.'—We have disposed successively of the doubtful judgments, the 'or-judgments,' and the judgment 'no-difference' on the ground that they can not ordinarily be given under a constant serial psychophysical disposition. We are left with the categories 'greater,' 'equal,' and 'less;' and it now behooves us to enquire into the nature of these remaining forms of judgment. It is logically conceivable that 'greater' and 'equal,' for instance, are not serially related, that a change from the one

category to the other involves an extra-serial change in attitude, and that, as a consequence, psychophysical correlation becomes an impossibility. We are assured, however, by the observers that the contrary is the case; in broad general terms, at least, the judgments 'greater,' 'equal,' and 'less' are equivalents belonging to the same order, an order to which the doubtful judgment, in all the forms in which we have met it, does not belong.

F, 2, greater: "Just like my other judgments." Equal: "Just as usual. Judgment automatically says itself." That is to say, F reports no difference between 'greater' and 'equal.'

F, 6, greater, following an 'equal': "Just exactly like last experiment, only I said 'greater,' automatically. That it sounded greater is all I can say."

F, 40, greater: "No hesitation; both sounds tremendously clear; nothing else new." Equal: "Same." Equal: "Same." Less: "Nothing new; just a little surprise."

Bo, 9, equal, after a 'greater': "Mechanical judgment like the last."

Bo, 14, greater, after an 'equal': "I can't see that there was any change of attitude from the preceding judgment to this one of 'greater.' . . . If the change in the situation is a change in attitude, it is at least not the definite change which I understand by attitude, such as a change to surprise, expectation, certainty, and so forth."

Bo, 17, equal, after a 'greater': "I don't think I can find any difference of conditions of judgment there. The 'equal' came out just as mechanically as the previous 'greaters' have done. I wasn't even aware that I was giving a different judgment until after I had given it. As far as attitude or conscious conditions go, the last two trials seem to me practically identical." Less: "Some delay. . . . *Less* came as mechanically and immediately as the *greaters* and *equals* have. It did not seem to be dependent on the delay, although the delay is doubtless dependent on the lessness of the difference. I do not now see any difference between the necessary conditions of *greater, equal,* or *less.*"

Bo, 20, less, after two 'equals': "I shouldn't say there was any gross shift of attitude from the last two times. The conditions for *less* appeared to lie absolutely in the visual impression. I see the lines and I say the judgment; that is about all it seems to be."

Bo, 22, equal. "There I had a change of judgment from *less* to *equal* that did not mean, I should say, a change of attitude. I was passive, attentively disposed, not doubtful at any time. I just made the first judgment and then changed it without the doubt or hesitation that I should call an attitude of uncertainty."

Bo, 26, equal. "Normal although I changed from *less* to *equal.* The judgment was normal in spite of the change because the change wasn't a change in my attitude but a change in the length of the lines—I saw the right hand line grow. I was not even disturbed by the change at the time, although afterward I was doubtful."

Bo, 31, equal, after a 'greater': ". . . I was surprised to find that I had changed the category so easily, practically without realizing it."

Bo, 39, equal, after seven 'greaters': "Normal. . . . I think this time my attitude was just the same when I gave *equal* as *greater.*

. . . I should say the visual appearance of the extents was the condition of change."

Bo, 41, equal, after five 'lesses': "I can't see that there is any change in the conditions of judgment. This time was just like last time."

Bo, 52, equal, after four 'greaters': "Normal. Entirely like last judgment in spite of the change of category."

Bo, 55, equal, after six 'greaters': "Normal. I haven't the least idea what made me change my judgment. The whole thing runs off like the very smoothest habit."

The foregoing are but a few excerpts from a great number of long reports made by Bo, all with the same import. Bo continued to seek vainly for the conditions of change between these three categories, and could find in general only the visual impression, which he could not regard as attitudinal. He was continually troubled by his belief that an attitude is essentially a nervous affair and may normally be unconscious and unreportable.

Bi, 15, greater, after an 'equal': "The same kinaesthetic experience meant *greater*." Equal: "The equality judgment is in many ways like the *greater* judgment. It has none of the feeling of doubt or hesitancy; the general pattern of the experience is the same, save that the predominant kinaesthesis which carries the meaning is different. Instead of the chest kinaesthesis of *greater*, I have kinaesthesis in the upper chest or neck which means *equal* and is practically all there is to the equalness of the experience."

Bi, 40, less, after an 'equal': "This was the same type as the one I just had."

R, 2, greater, after an 'equal': "The only condition of change which I can find is the change in the relation between the two stimuli."

R, 25, less, after a 'less': "No change from last at all." Then equal: "The only condition I can find for the change is the difference in the stimulus and the sensation." Then less: "Just the same kind of difference between the two categories." Then less: "No change at all." Then greater: "The condition of change from one category to another was only a change in the sensation that gave rise to the judgment." Then equal: "Same conditions of change as reported last time."

R, 61, less, after an 'equal': "The only condition of change is a change in the stimulus. Both are so automatic that there is nothing conscious in them to change."

On several other occasions R made reports which are almost identical with this last one.

The category 'equal' appears to be more equivocal than the categories 'greater' and 'less.' On the one hand, 'equal' does constitute a positive category, comparable to 'greater' or 'less.'

F, 17, equal. "Second sound came as identical with the first." And thereafter F used the term *identical* to represent this special kind of *equal*.

R, 4, equal. "The feeling that the two lines were equal came quickly. It was a kinaesthetic image which definitely carried the meaning *equal*."

R, 58, equal. "The kinaesthetic feeling of *equal* came in."

R, 77, equal. "A typically different form of judgment [from 'no-

difference']. Instead of the kinaesthetic feeling of uncertainty, I know these things to be equal as if there were *equal* in them."

On the other hand, the 'equal' judgment is sometimes a doubtful judgment:

R, 40, equal. "The *equal* judgments seem to possess a little of a doubtful character. I am not as confident of them as I am of the *greater* or *less* judgments."

Other observers hint at the doubt. It appears that sometimes 'equal' is given when the judgment should have been 'greater-or-less,' a typical doubtful judgment of the 'or' type. It should be remembered further that the range of equality is limited in both directions, and that it is therefore impossible to have as striking cases of positive equality as it is of positive greaterness or lessness. Between the equality which is doubt and positive equality there are degrees of hesitation which can not always be classed as doubt:

F, 18, equal. "There are degrees of definiteness of *equal,* I think. At least I am more certain of my *equal* judgments when there is identity than when there is not."

F, 28, equal. "They were nearer identical (not absolutely identical) than the last *equal.*"

F, 36. "There are two kinds of *equal* judgments. There is identity where you automatically say *equal;* organic relief and an immediate judgment. Then there is the *equal* judgment which comes when you are struck by the difference of the two, but when you don't know whether they are different in intensity or in something else."

We should, therefore, keep explicitly in mind that the equivalence of the 'equal' judgment to the 'greater' and the 'less,' an equivalence which the reports attest, can probably be stated only of the positive judgment of 'equal.' There is a more or less doubtful judgment, which is sometimes masked by the term 'equal,' and which must be excluded with the other doubtful judgments.

The reaction-times for the three positive categories (Table VI; less = 0.88 sec., greater = 0.92 sec., equal = 1.10 sec.) are less than the times for any of the doubtful categories (except the mixed doubtfuls). The long time for 'equal' is probably due to the inclusion of a number of 'doubtful equals.' Hence the difference in time between the positive categories and the doubtful categories is in reality greater than here appears.

Variations Within the Serial Attitude.—We have dealt with the categories 'greater,' 'equal,' and 'less' in the gross, and we have found that they can be regarded as co-ordinate terms within a series, and that the psychophysical organism can apparently assume a constant disposition over against the

entire series without varying in attitude from term to term. By speaking thus generally we have not meant to imply that all judgments of a single category are identical, and indeed we have given warning that the term 'equal' may stand for very different judgments. Within the serial attitude there occur degrees of 'greater' and degrees of 'less,' possibly even degrees of 'equal.' We might dismiss this fact as of little importance were it not true that the various degrees of difference appear to be correlated with degrees of immediacy or delay, and delay is often described as 'hesitation,' and 'hesitation' sometimes means doubt. But doubt arises from a departure from the serial disposition, transgressing the instruction. Let us turn to the reports.

Of these degrees F has the most to say, but Bo, Bi, and E also admit their existence.

F, 11. "The judgments naturally seem to be either *greater, equal,* or *less.* It is rare that I get a *no-difference* judgment. But there is a difference among the *equal* judgments and among the *greater* and *less* judgments in the assurance or definiteness of the judgments."

F, 17. "I find considerable tendency to tell you 'how much' less very often. I think I could take a scale of 5 or 10 and say in terms of that scale how much less the sound was."

F, 18, greater: "Quite surprised. The second sound was very much greater." Greater: "Very much greater. The first was absolutely rather weak." Greater: "about 2 in a scale of 10." Greater: "Tendency to say 'not very much.'"

F, 25. "I can't see anything in the instructions to forbid me from giving these judgments on the basis of a scale of 10; so I have decided to try to get a serial set of judgments by estimating on a scale of 10. Thus, if I say 'less 10,' it means about as different as I ever get, *i. e.* the judgments are so-much *quantitatively* less. *Equal* would be 'less 0.' I don't seem to find doubtful judgments or 'or-judgments' except very rarely."

F, 26, equal: "Just a little hesitation and extra comparison. The two weren't quite identical."

F, 33, greater 8 (F is using his scale of degrees): "A quite definite organic reaction that goes with certainty and with judgments where the difference is greater than 4, say, on my scale. A different organic complex goes with hesitation, uncertainty, a not immediate judgment, such as when the difference is only 1 or 2. With the former goes the tendency to shout the judgment out; with the latter the tendency to put a question-mark in my voice and to speak slowly rather than quickly. When the judgment is 8 or 9 there is also surprise. When the judgments are around 1 or 2 I am pretty much always confused, perhaps hesitant."

F, 36. "I think you can make several distinctions of judgment: (*a*) Judgment surprisingly less, with a little shock to it. (*b*) Definite judgment, where one is automatic about giving it. There is little organic stir up; it is just normally *less.* (*c*) Judgment where there is strikingly little difference. The organics are different in these three cases. In the first case they seem to be surprise or shock. In the second they *mean* 'greater' or 'less.' In the third they are differ-

ent; there is strain in them, always strain with these smaller differences."

Bo, 44, greater. "Normal, delay. Of course there is a difference in the amount of difference between extents. I could easily make different categories of degree, say 'much greater,' 'slightly greater,' and 'barely greater,' for example; but I do not regard these categories as different in the way that is called for; they do not seem to be discrete. On the other hand, I am somewhat puzzled about them because they do seem to play a part. . . . I merely want to call attention to the fact that under the same attitude I may have very different experiences, which vary, on the side of impression, in degree of greaterness or lessness, and, on the side of subjective supplementation, in the amount of delay and the consequent patterning of consciousness, a patterning, which, in some cases, seems closely related to the attitude of doubt."

Bo, 76, equal. "Normal, but a little hesitation similar to doubt in which I wondered whether it was *equal* or *less*. Immediately after the trial I thought to myself that the difference between 'normal equal' and 'hesitant equal' is largely a difference in the appearance of the lines. When I became hesitant, I seemed to get the lines as 'less' and then as 'equal.' In other words, being doubtful in this way does not seem to be the sort of thing that doubt usually is."

Similar to F's degrees are Bo's reports of 'immediacy' and 'delay.' Bo fell into the habit of regularly reporting 'normal' judgments with phrases like the following: "Normal, immediate;" "Normal, delayed;" "Normal, slight delay;" "Normal, fairly immediate;" "Normal, perhaps a trifle hesitant." Thus immediacy and delay are always given as irrelevant to the normality of the judgment.

Bi, 70, greater: "Quite obviously *greater*, but not extremely obvious. I think *greater* was carried by a denser blackness." Greater: "Somewhat *greater*. Attention good. No definite imagery of any sort. I was pretty doubtful, because of the 'somewhat,' I guess."

E, 16, greater. "Very decidedly greater."

E, 26, less: "That seemed decidedly less. A sort of relief about it; hence easy to judge." Less: "The difference didn't seem as great. It was definitely less; still there was a different feeling about it from the last one."

E, 49, greater: "Just definitely *greater*." Greater: "No change, though the pair seemed nearer together. In both trials an attitude of assurance and satisfaction."

E gives a number of similar reports of degrees.

The evidence that there are more than three degrees within the series 'greater-equal-less' is complete. F uses a scale of 21 degrees (actually he employed but 16, however; from 'less-8' through 'equal' to 'greater-7'), but suggests that qualitatively there might be seven, three 'lesses,' an 'equal,' and three 'greaters.' The other observers are not so explicit.

The smaller degrees of 'greater' and 'less' are correlated with 'non-immediacy' or 'delay' or 'hesitation' or 'delay with alternation of judgments' or 'doubt.' It is not clear whether this state is properly to be called 'doubt' or not. It is certain that it is not the sort of doubt which we met in the *doubtful* judgments. That doubt was extra-serial and

meant a change of attitude. This takes its place within the series; its limens could be, if necessary, determined; it therefore comes within the instruction and is allowable in psychophysical method.

Bo is especially insistent upon the intra-serial nature of 'hesitation.' We quote a few of his reports:

Bo, 9, equal. "A hesitation before spoken judgment. I don't think 'hesitation' is quite the same thing as 'uncertainty,' although it is similar. 'Hesitation' is the being unable to give a judgment, whereas 'uncertainty' is the being unable to give a judgment because there is an alternation between tentative judgments."

Bo, 17, greater. "Passivity, then delay, then mechanical judgment. I think this delay isn't quite 'passivity,' because it involves searching for a way out. I am always restive under 'delay.'"

Bo, 34, equal. "Considerable delay, and something like an alternation of judgment during delay, hardly more than a strain in my chest. It was not doubt or hesitation; thus I can say my attitude was 'normal.'"

Bo, 35, equal. "I am under a tremendous logical suggestion to call any temporal hesitation or any alternation of judgments 'doubt;' but I am consciously trying to resist this suggestion because 'doubt' is an attitude and ought, therefore, in the sense in which I am using it, to feel 'doubty.'"

Bo, 41, equal. ". . . There was no 'doubt;' just hesitation and delay."

Bo, 45, equal. "That was a good normal judgment, although quite delayed." And so on.

Bi at one time equated 'slowness' of response to the 'doubt' of expectation, but later reversed his opinion:

Bi, 27. "What I call 'doubt' is slowness in accepting the judgment. It seems as if I might have had expectation and become conscious of it when the variable was given."

Bi, 38, less: "That was normal. I seemed to wait without any expectation at all. Ordinarily when I wait I seem to expect."

We may support the direct testimony of the observers as to the continuity of the successive degrees of the series 'greater-equal-less,' by an examination of F's reaction times for the various points on his judgment-scale (see next page).

Although the cases involved are very few, the curve of the times gives every evidence of being a continuous function. In fact, if it were to be regarded as discontinuous at any place, the breaks should be made between 'greater-1' and 'greater-2' and between 'less-1' and 'less-2,' for the smallest values of 'greater' and 'less' seem to group themselves more with 'equal' than with the other values of 'greater' and 'less.'

We have noted in our introductory discussion of the problem which the present experiment attacks that the most definitely recognized form of intra-serial variation is the constant

Category and its degree	Number of cases involved	Average reaction-time (seconds)
1–8	6	.74
1–7	10	.74
1–6	9	.90
1–5	5	.82
1–4	2	.68
1–3	7	1.08
1–2	3	1.00
1–1	2	2.00
=	55	1.94
g–1	2	1.90
g–2	6	1.06
g–3	7	1.06
g–4	5	.92
g–5	5	.96
g–6	10	.82
g–7	6	88

change of disposition of the organism, over against the stimulus, which is taken account of in the psychometric function. In other words, the same stimulus under the same external conditions can not be expected always to give rise to the same impression. The reports of our observers are full of accounts of this kind of change. Many examples already quoted (*e. g.* Bo, 76, immediately above) show how the impression changed while the stimulus remained constant. At another time Bo reported:

Bo, 26, equal. "Normal, although I changed from 'less' to 'equal.' The change was a change in the length of the lines; I saw the right hand line grow."

Bo, 54, less. "Normal. The right hand line got very much less while I was making the judgment."

The Normal Type of Judgment Under the Constant Attitude.—All the observers tended, in the effort to maintain a constant receptive attitude, to make passive judgments. The passive judgment, moreover, tends to become immediate, although it is not necessarily immediate. The implication is that the way to maintain a constant attitude is to dispose oneself to make passive, immediate judgments. A few quotations, in view of the many which we have already given under other heads, will suffice to show the normal type of judgment.

F, 13, less. "I am open-minded for any judgment. The judgment just comes immediately. That is all."

F, 28, greater. "First impressed me as a little greater than those of the last series. Second came as immediately greater." These

reports occur in spite of the fact that F tended naturally to be reflective and to assure himself of his judgment (and sometimes of its degree) before he reported.

Bo, 55, equal, after a 'greater.' "Normal. I haven't the least idea what made me change my judgment. The whole thing runs off like the very smoothest habit."

Bo, 74, less. "Normal. What happened was that I saw the lines, and that, after a moment, I said *less* in the same smooth automatic manner."

Bi, 23, greater. "Perfectly indifferent, mildly attentive, no expectation at all. The standard was carried in an image until the variable came. Then just a step added to it, which meant *greater.*"

Bi, 29, greater. "That was an automatic judgment. No attention to standard or variable. I just found myself speaking judgment in internal speech."

R, 18, less. "The kinaesthesis which carried the meaning of the judgment was very faint. Very little that was conscious in judging."

R, 24, equal. "A little more immediate and automatic than the last. Little conscious about it."

R, 87, equal. "That judgment was almost unconscious, practically automatic. The whole mechanism of kinaesthesis has dropped out." And yet this judgment represents a change of category from *no-difference* to *equal.*

E, 5, equal. "Judgment popped out instantly; it just came."

E, 8, equal, after a *greater:* "Just simply snapped right out the minute I heard the second sound."

E, 26, greater. "That just came popping right out; perfectly definite judgment; that was all there was to it."

The prevalence of the passive, immediate judgment marks it as the type. Furthermore some of the observers (most notably Bo) adopt it intentionally as a means of meeting the requirements of the instruction. F and Bi regard it as the normal attitude.

F, 36, less. "I said *less* before I realized that the series was 'equal-or-greater.' The judgment came automatically. . . . A definite judgment is where one is automatic about giving it. That is just normally *less.*"

Bo, 11, less. "I am trying to be passive and mechanical as this attitude seems the most likely to remain constant."

Bo, 22, equal. "I am trying to keep my attitude constant by judging immediately, smoothly, and automatically. I think that an indecision or change of judgment is a difference in the attitude."

Bo, 26, less: "That was normal, *i. e.* passive and automatic. I don't mean that there was a lack of attention when I say automatic, but that there was a smooth course." Less: "Normal and immediate. Immediate is the opposite of delayed. The procedure may be normal, but the meaning comes in slowly."

Bi, 37, equal. "This is my 'standard' attitude: indifferent, fully attentive."

Opposed to the immediate, passive, normal judgment is the 'reflective' judgment, which is by implication active and delayed. Most of the observers provide examples of this type, although F is the most prone to adopt it. (*Cf.* F's reaction

times, Table IV, which are much longer than those of his fellow observers.)

F, 2, greater. "The second sound was bigger, duller, thicker than the first, and something representing that bigness was in mind as soon as the second was over. After that I reviewed the experience in sensation and imagery; perhaps in kinaesthesis too, and then I felt kinaesthetically that the second was the more intense."

F, 27, greater-or-no-difference. "I wanted to say *greater-or-equal* instead. When the sound came I was immediately doubtful, as the second sound did not come either as different or not different. Then it seemed to me a little greater; then it seemed equal; then I wanted to say *greater-or-equal.* Then I thought of my instructions and it seemed to me as if *greater-or-equal* might not be in a serial series. Then it struck me that *greater-or-no-difference* satisfied the conditions pretty well, because, although the second sound did not come to me as positively 'not different,' neither did it come as positively 'different.'"

And so in many of F's reports; but they are too long to quote.

Bo, 58, equal. "I was starting to say *greater;* then hesitated and changed to *equal* because I knew *greater* was unlikely. This is not 'normal.'"

Bi, 59, equal-or-less. "I'll say *or-less* because I expected *greater.* The expectation hung on so that I judged the variable against my expected variable as if it were the standard. At least one of my two judgments was a comparison between the variable and the expected variable."

E, 16, greater. "At first it seemed as if I ought to say *greater-or-equal* or *equal-or-greater* perhaps, to get an *equal* into it in some way; but I immediately rejected that because the tendency to say *equal* was just a determination from the last experiment."

The reflective attitude is more complex than the passive; naturally, then, it gives more opportunity for variations. The doubtful attitudes, which we have found it necessary to exclude, are but forms of reflective attitudes. One dare not state dogmatically that a reflective attitude can not be maintained as constant. On the other hand, there is no evidence that it can. The safe rule in psychophysical work upon simple sensory judgments would seem to be to restrict the observer to a passive, immediate type of judgment, and to exclude all other forms by specific instruction, by the control of conditions, and, if need be, by the rejection of failures in the manner in which the failures of attention are ordinarily rejected.

The great enemy of the immediate, passive judgment is the stimulus error. The reflective judgments occur most readily when the observer is judging objective lines or objective sounds, is trying, in a sense, to be right. The impressions change, under the dispositional variations within the psychometric function, so that in a delayed judgment a single category can seldom represent the impression; hence the premium

placed upon judging stimuli rather than impressions. The immediate judgment simplifies the impression by cutting down the opportunity for temporal variation; the passive attitude creates a stable disposition. The two together tend to reduce the psychophysical correlation to its simplest terms and to render it meaningful as a scientific datum by keeping all of its conditions constant except the variant under investigation.

The Term 'Attitude.'—We may be accused of using the term 'attitude' in this paper in an indefinite sense. It is true that we did not define the term to our observers, nor have we defined it since. All the observers objected to it at first, complained that they did not know what was meant, or stated that since an attitude might be unconscious they could not be expected to report upon it. Our justification for the term lies, however, in the outcome of our work; at the end of the experiment we find the observers reporting constancy of attitude and change of attitude as a matter of course. They name and even partially describe attitudes. They still assert that attitudes are sometimes unconscious; and consequently they are still concerned lest certain attitudes, being unconscious, should be unreportable. There can be no doubt, however, that the word came to have a real meaning, and that the positive results of our present study reflect an intelligible use of the term.

We must still, however, postpone the definition of the word. Undoubtedly attitude is psychophysical. Obviously, also, it is sometimes but not always conscious. The maintenance of a constant attitude becomes at least partially feasible because there turn out to be three ways in which we can know something about attitude in general. In the first place, a new and intruding attitude may be conscious, and as such observed and reported by the observer. In the second place, a change of attitude may not be immediately conscious, but may make itself felt after the fact by some new and unusual conscious feature. For example, the observers sometimes suspected 'expectation' because they found they had given, automatically, a category which they could explain afterward only by supposing the influence of expectation. Thus we may learn of attitudes from the reported inferences of observers. Finally, the reaction-times furnish an objective clue. A wide departure from the normal reaction-time for a given variable stimulus is a strong presumption in favor of an attitudinal change, whether conscious or unconscious.

We must add that the recognition of the gross factor which we have referred to as attitude or disposition occurs in almost

any major piece of psychological work. We read in the literature of the effects of distraction, the need of avoiding distraction, the necessity of judging with maximal attention; we are told that this observer altered his criteria of judgment and that that observer used a secondary cue; we find the literature of the metric methods full of reference to expectation, habituation, and fatigue. All these discussions imply that an attempt is being made to keep the psychophysical machine constant or to take account of its inconstancy; but no one defines an attitude. And the analysis of the psychophysical disposition must wait for further investigation. Meanwhile we have demonstrated that it is possible to use the term in the gross and still to achieve results which may contribute toward the standardization of the conditions of the psychophysical experiment.

Conclusions

It is possible in psychophysical work for an observer to maintain a constant disposition or attitude over against a particular series of impressions.

Doubt and expectation and their derivatives usually violate this constant attitude. The 'doubtful judgments' of psychophysics, the 'or-judgments,' and the judgment 'no-difference,' all imply the introduction of a form of doubt which lies without the serial disposition, and must therefore be excluded from the ordinary psychophysical computations.

The judgments 'greater,' 'equal,' and 'less' may all occur under the same constant serial disposition. Various degrees of 'greater' and 'less' may also be given under the constant attitude.

The immediate and passive type of judgment is favorable to the maintenance of constancy of attitude. A reflective judgment, on the contrary, favors dispositional change.

If, then, our results are confirmed by other investigators, it will be advisable in future psychophysical work so to frame instructions and so to direct preliminary practice that constancy of attitude and immediacy of judgment become natural and normal to the observers. The end cannot be attained by prohibition of the 'doubtful' judgment or by the requirement that the observers guess at a difference in every case; for these instructions secure constancy of the expression of judgment at the cost of instability of attitude. The observers must rather be trained in such a way that the experimental situation, while it may prompt to the response 'delayed equal,' *e. g.,* shall not suggest the response 'doubtful.' If with some observers or under certain circumstances the *doubtful* judgments persist, they must be made the subject of special study on their own account, and not included in the data used for the regular computation.

THE SIGNIFICANCE OF STIMULATION IN THE DEVELOPMENT OF THE NERVOUS SYSTEM

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The unit in the nervous system is the neurone. It is now pretty generally accepted that this unit—made up of the cell body and its processes, the protoplasmic processes, or dendrites, that convey nervous impulses to the cell body, and the nerve fiber, axon or neurite, that conducts nervous impulses from the cell body,—is an anatomical unit, a functional unit, a trophic unit, and a genetic unit. The nervous tissue is made up of millions of such neurones. But all are functionally connected and integrated into a system. The child has as many neurones as the adult, but they are undeveloped.

It has long been known that stimulation is a condition of development in the nerve centers. The classic investigations by Held and others have shown, too, that stimulation hastens development. More recent studies have shown not only that the development of the nervous system in the individual and in the philogenetic series has been conditioned by stimulation from the external world, but also that function and stimulation have been fundamentally significant in the development of the individual neurones. It is the purpose of the present paper to trace briefly some phases in the development of the nervous system as determined by stimulation.

Our knowledge of the genesis of nervous tissue is still inadequate. It seems, however, most in harmony with the knowledge we have, to suppose that it developed *pari passu* with the growing complexity of reactions to the stimuli of the environment. Thus with the reactions similar to the tropisms and the like in primitive animals there developed a mechanism for coördinating such reactions. In general this development has taken place in connection with the function of reacting to stimulation. In certain lower orders of animals, the corals, for example, nerve cells and fibres are evenly distributed to the whole surface of the body. In other animals, those parts most favorably located for receiving impressions from external objects become more richly supplied with nerve cells (29).

In the medusa this location is the rim of the bell. In the echinoderms it is around the mouth, and here is developed a

ring of nerves. The bell rim of the medusa, and the oral ring of the echinoderms with its radiating paths, represent a central nervous system with which the rest of the nerves may be contrasted as peripheral. Thus the nervous substance originates in contact with the stimuli of the external world.

Again, in the embryo the nervous substance is formed, not from the mesoblast, but from the outer germ layer, the ectoderm or epiblast, in response, as it were, to external stimuli, and then folded in to form the nerve tube, the original form of the central nervous system.

Thus both in the individual and in the phylogenetic series the nervous system seems to have been developed under the stimulus of the external world. As the nervous system becomes more and more complex we find certain special structures or special organs developed in connection with the reaction to certain stimuli; and finally, we have the marvelous mechanism of the human brain and its connections with the external world.

From the functional point of view Herrick (12, p. 36) has summarized the evolution of the nervous mechanism as follows:

> "The functions which characterize the nervous system have been derived from those of ordinary protoplasm by further development of three of the fundamental protoplasmic properties, viz., sensitivity, conductivity, and correlation. The most primitive form of nervous system known is diffuse and local in its action, but in all the more highly developed forms the chief nervous organs tend to be centralized for ease of general correlation and control. Most of the types of nervous systems found in the animal kingdom are represented in two distinct and divergent lines of evolution, one adapted especially well for the reflex and instinctive mode of life and found in the worms, insects, and their allies, and the other found in the vertebrates and culminating in the human brain with its remarkable capacity for individually acquired and conscious functions."

The great function of the nervous system from the physiological point of view is the coördination and integration of movement and the adaptation of reaction to sensory-motor experience. This has been shown in detail by Sherrington and others (23a).

As has often been pointed out, the nervous system is a mechanism for converting stimuli into reactions. It has been customary to look upon the reflex arc as the type of all that is accomplished by the nervous mechanism. The essential parts of the process are three: first, an incoming nervous current along an afferent or sensory nerve; second, a central process more or less complicated; third, an outgoing nervous current along an efferent or motor nerve to innervate a muscle

or a gland. All the accomplishments of the nervous system have usually been described as reflexes or combinations of reflexes.

To take a concrete case, the classic illustration is as follows: If one puts a drop of acid on the thigh of a decapitated frog, immediately the leg is drawn up in the attempt to brush off the irritating substance. Here we have an example of the simple reflex arc. The course of the nervous impulse is briefly as follows: The nervous impulse caused by the stimulation of the nerves of touch in the frog's thigh is carried by the afferent sensory nerves to the sensory cells in the dorsal part of the cord, then the impulse is transferred by the central nervous processes to the motor ganglion cells in the ventral part of the cord, and the excitation of them causes an outgoing nervous current along the efferent or motor nerve which innervates the muscles of the frog's leg.

The essential elements of the anatomical structure involved in a reflex are three: first, a receptor or sensory receiving organ; second, a connector, or adjustor, the nervous tissue connecting the afferent nerve with the efferent or motor nerve; and third, the effector, consisting of the mechanism of response. Or we may divide the mechanism more concretely as Herrick (12, p. 25) does, into five parts as follows:

"(1) A sensitive receiving organ (receptor or sense organ); (2) a conductor (afferent or sensory nerve) transmitting the nervous impulse inward from the receptor; (3) a correlation center or adjustor, generally located within the central nervous system; (4) a second conductor (efferent or motor nerve) transmitting the nervous impulse outward from the center to (5) the effector apparatus, consisting of the organs of response (muscles, glands) and the terminals of the efferent nerves upon them."

The sequence in the development of this mechanism is not altogether clear. The studies of sponges and other lower forms of animal life by Parker and others, however, indicate that the muscle preceded the nerve in its evolution and was the first element of the reflex arc to be developed. Sponges, for example, represent animals with effectors without differentiated receptors. Evidence is furnished also by certain reactions in the higher animals. To quote Parker (22, p. 124):

"If muscle originated before nerve and was brought into action at first by direct stimulation, it is natural to expect that examples of this form of response might still be found among the higher animals. And such seems to be the case. Thus the sphincter of the iris in the lower vertebrates, though well known to be under the influence of nerves, was shown by Steinach some time ago to be directly stimulated by light, a condition which, judging from the more recent work of Hertel, probably applies even to the human eye. This muscle then exhibits a certain

capacity for normal direct stimulation. Another example of the same kind is seen in the embryonic vertebrate heart. Though the beat of the adult heart may be a matter of controversy from the standpoint of the myogenic and neurogenic theories, there can be no doubt that the muscle of the embryonic heart beats, as shown by His, before it has become invaded by nerves. And this view is supported by Barrow's recent discovery that the isolated cells of the heart-muscle will contract rhythmically under conditions where not the least vestige of a nerve can influence them. Thus the embryonic heart-muscle and the sphincter of the iris are muscles whose activity may be normally called forth by direct stimulation, a condition which reproduces, so far as independence is concerned, the state met with in the muscles of the sponges. These examples then show that even in the higher animals certain muscles respond normally to direct stimulation and thus exhibit a form of activity which is believed to be generally characteristic of sponges."

A good example of a very primitive form of nervous system is that found in the sea-anemones. As described by Parker (22, p. 121), these are sac-like animals with a single opening leading into the digestive cavity. The animal has no well-defined organs except the digestive apparatus and is merely a membranous digestive sac. The body of the sea-anemone, however, contains both nerve and muscle, occurring over almost the whole of the animal. Each part seems to carry its own neuro-muscular mechanism. The nervous system in the sea-anemone is diffused rather than centralized. No part of the animal's nervous organisms seems to be more important than any other part; it consists of a vast number of sensory neurones which connect the surface of the animal with the underlying muscles. Its function is merely the reception of stimuli and the immediate excitation of the muscles. The nervous mechanism is a receptor that acts as a trigger for setting off the muscle. That is, the animal consists merely of receptors and effectors without the intervention of an adjustor or a central organ.

Afterward as these receptors became more highly developed, the third element, the central nervous element, was evolved. This serves as an adjustor, a means of connection and correlation between the receptor and the effector. Thus the order of development was: first, the effector; second, the receptor; and third, the adjustor or central organ.

Thus the nervous system originated "at spots," probably where the animal had developed muscle, and was later unified. As Parker has expressed it (22, p. 127):

"Although the nervous system probably arose in a scattered way at spots where the primitive multicellular animal had developed muscle, it became unified through the need for general transmission tracts, and, by increasing its own elements as well as by appropriating additional

effectors and receptors, it has impressed upon the higher animals, including ourselves, a unity so profound that it includes everything that we mean by personality."

Another class of recent investigations show that there are kinds of nervous action not adequately described as reflexes at all. Hough, in a recent paper, distinguishes the following classes of nervous action: automatic, axon reflex, unconditioned reflex, conditioned reflex, and volitional. For the present purpose it will be sufficient to note the automatic, unconditioned reflex, and conditioned reflex action of the nervous substance, and here we may well follow Professors Hough and Brown (14, p. 410):

"In the central nervous system the best known and most successfully studied case of automatism is that of the respiratory center. The conclusion which Rosenthal drew from his experiments, that the nerve cells of this center send out rhythmic discharges when removed from all connection with afferent nerves, has been confirmed by all subsequent work, the experiments of Winterstein being especially conclusive on this point."

"The work of T. Graham Brown suggests that the same thing is true of the rhythmic movements of locomotion. Brown shows that in a certain stage of ether narcosis in the decerebrate animal, when reflexes can no longer be elicited from the afferent nerves, rhythmic movements of flexion and extension occur in the hind legs; and furthermore, that these movements occur after the afferent nerves from the moving limbs are cut. In other words, these movements which suggest the basis of the movements of locomotion, involving as they do the alternate rhythmic action of antagonistic groups of muscles, are executed by efferent neurones without any stimulation from afferent neurones. They constitute 'an endless chain,' but not an endless chain of reflexes."

Brown (14, p. 411) raises the question whether these automatic actions of locomotion do not present a more primitive form of nervous activity than the reflex. He suggests that "the nervous mechanism of locomotion, like the nervous mechanism of respiration, is fundamentally an automatic mechanism. Later on afferent neurones are added to it, comparable to those of the pulmonary branches of the vagus. In this connection it is most significant that in general the same conditions so frequently referred to as stimuli of the respiratory center—lack of oxygen, excess of carbon dioxide, etc.—are the very conditions found to favor the movements of narcosis progression."

Apparently in such automatic nervous action we have nervous action without stimulation. The exception is, however, strictly more apparent than real. It would, perhaps, be better to say that in case of automatic nervous action the stimulus is within the central nervous organ itself instead of being carried to it by a receptor organ. Especially if we take

Verworn's definition of a stimulus (27), as any change, we are justified in taking this view. The automatic nervous action is like all other nervous action, response to stimulation, but in this case the stimulus is internal.

As the highest form of nerve action we have the association of stimuli in the production of conditioned reflexes. As shown by the Russian school of Pavlov, in the higher animals the stimulus from any receptor organ may become associated with the natural or biologically adequate stimulus that produces a given reaction and produce the same reaction. This is called a conditioned reflex in contrast with the ordinary or unconditioned reflex. The difference between the one and the other may be very simply illustrated.

If you give your dog a piece of meat a secretion of saliva occurs. This is an ordinary reflex. If every time you feed your dog meat you ring a bell, after a little while you can ring the bell without giving the meat and there will be a flow of saliva. The ringing of the bell has become associated with the stimulus of the meat and produces the same reaction. Such an associated stimulus is called a conditioned stimulus, and the reaction produced, a conditioned reflex. This is a most remarkable phenomenon. An entirely indifferent stimulus associated with the ordinary stimulus produces the same physical effect. This association is effected by the brain cortex.

Thus, as the genetic sequence for the elements of the reflex arc is: first, the effector; second, the receptor; and third, the adjustor; so in its simplest form the genetic sequence of the different forms of nervous action is probably as follows: automatic action, reflex action, conditioned reflex action.

A helpful distinction for the genetic point of view is that between the paleencephalon, or old brain, and the neëncephalon, or new brain, sometimes called the pallium or brain mantle. As Edinger (5, p. 12) expresses it: "The most important thing of general significance which comparative anatomy teaches is this, namely, that the whole mechanism of the spinal cord up to the olfactive nerve is in case of all the higher and lower vertebrates similarly organized; that therefore for the simplest functions through the whole series a similar basis exists whether it is a question of man or fish. This fundamental part of the brain, the old part, we may call the paleencephalon." In contrast with this is the more recently developed part of the brain, the neëncephalon, or the brain cortex and its dependencies.

Neëncephalon in black
Paleëncephalon in gray
(After Edinger)

The former, the paleëncephalon, was the original brain. It consists of the cerebellum and the brain stem; and the neëncephalon consists of the part of the brain superimposed on these basal portions, the cortex and its dependencies. Hence the name brain mantle.

The paleëncephalon mediates movements, the reflexes, and many instincts. Locomotion, posture, the processes of nutrition and other vital processes are controlled by it. The neëncephalon, on the other hand, mediates the functions of association and the higher mental processes.

Not only the old brain but also the new brain has developed in behoof of adjustment to the environment. In Edinger's words (6):

"Wherever we investigate, everywhere the development of separate parts of the paleëncephalon appear dependent altogether on the manner of life. Traces of the neëncephalon, an apparatus which folds itself over the paleëncephalon, appear in the case of selachians, and become greater in case of the amphibians. In a continuous series it increases through the amphibians up to the mammals, and in the latter it develops to the enormous structure which in case of man becomes the bearer of all the higher mental functions. If we divide all actions into those of the paleëncephalon and those of the neëncephalon according as they are performed by one or the other of the two great divisions of the brain, we acquire a principle of classification and study for comparative psychology."

While no hard and fast divisions occur in neurological development, any more than dramatic epochs are found in psychological evolution, we do find with the appearance of the neëncephalon a new form of behavior, and new possibilities for the development of intelligence. Fishes have only the paleëncephalon. The rudiments of the neëncephalon are found in selachians and amphibians. The neëncephalon appears definitely in reptiles. And as Edinger (6, pp. 453-4) says:

"Finally, in the mammals we meet a brain which has so large a neëncephalon that we may well expect a subordination of reflexes and instincts to associative and intelligent actions. That, in fact, is the case with those mammals in which the neëncephalon includes much more than half the bulk of the entire brain. But in many families there is very little advance beyond the condition prevailing in birds, for example, in the hedgehogs and the moles. In the mice, the rabbits, in fact in nearly all the rodents, the two parts are about evenly balanced. What we know of the intelligence of these animals—and that is little enough—is in very close accord with the condition of the brain."

Again, as Edinger (6, p. 447) has pointed out, a distinct advance appears with the advent of the neëncephalon. This change of behavior is especially noteworthy in the activities related to the taking of food.

"Hungry animals," he notes, "if they possess only the paleëncephalon seize food under all circumstances, provided the stimuli which proceed from it are appropriate, but only then. An animal which is incited to seizing only by a moving body never recognizes the same body if it is at rest. All of these animals can be caught with bait if one has ascertained the proper stimulus. Fishes which, like the trout, go toward

swiftly moving and glittering insects can be caught by imitations of such insects constructed of metal and feathers, providing the angler rightly imitates the hopping movement. The entire art of angling, concerning which we possess large volumes, depends upon knowledge of the proper stimulus and upon the excluding of disturbing stimuli, such for example as a thick fish-line. Frogs may be caught by means of heath-berries dangled before them on a string. Even the frog has a rudimentary neëncephalon, but so far as my observations go, it plays no part in the obtaining of food. It still eats paleëncephalically. No matter how hungry a frog may be, it seizes the earthworm only when it crawls, or catches the fly only when it makes some movement. One may lay a worm on the frog's snout or may in any way bring the two in contact, but eating does not result. The worm acts as a stimulus only when crawling, otherwise it is not recognized."

Thus while the paleëncephalic animal is stimulated by its prey only when the latter moves, serpents, on the other hand, once stimulated by a jumping frog or a running mouse follow their prey, at least for a time, and are able, perhaps by the olfactory sense, to find a particular hole into which the prey has crept. Lizards and serpents also assume an attitude of defense when danger threatens. They direct the head toward the enemy and attempt to bite. Edinger says he has never observed anything of this kind in a paleencephalic animal. Among reptiles also for the first time one meets with individual differences; in the same species there are indolent and excitable individuals; this, too, is probably due to the neëncephalon. And finally, reptiles learn more easily than fishes. Again to quote Edinger (6, p. 450):

"Most important in the psychological behavior of reptiles is the fact that the animals are no longer always dependent upon the sense impression of the moment, but that earlier impressions influence them. Further, they associate certain sense impressions which lie within the realm of the olfactory and oral senses, and turn them to account; they learn more easily than fishes and amphibians; occasionally they foresee; and they exhibit individual differences. There can be no doubt that all of these facts are referable to the appearance of a cortex in the neëncephalon."

Edinger sums up his view of the relations between the structure of the nervous system and its activity in substance as follows (5, p. 221): The paleëncephalon functions impressions and motions, often of a very complicated sort. Above these is developed in continually increasing degree with the growth of the cortex the ability for knowing and doing. This has its basis in the sense centers of the cortex, an apparatus to which we must ascribe the ability of coördinating the impressions that come from the paleencephalon with numerous others in such a way that it can inhibit and again reproduce them if similar or related impressions stimulate it. Thus knowing leads to doing.

Separate parts of the cortex are more directly connected with the peripheral sensory apparatus, others more with the peripheral motor apparatus. With these parts of the brain are associated other areas of the cortex, especially in the frontal lobe; and with the appearance of this, the intelligence first appears clearly, along with knowing and doing.

The reactions brought about by the paleëncephalon in response to stimuli, the actions which proceed from the sense centers on occasion of perceptions, are similar in case of man and animals. Indeed, the animal is sometimes far superior to man in both of these. Only one thing is developed in case of man far more than in case of animals, namely, the association centers, especially the frontal lobe; and with this, high intelligence, which presupposes consciousness; but since the frontal lobe is present in different degree even in animals, we are forced to the assumption that many actions of animals must be accompanied by the functioning of this part of the brain. "Comparative anatomy," says Edinger, "becomes here the pathfinder of psychology."

An important point not always noted is the increased scope and importance of cortical control in the higher animals, especially in human beings. The child reported by Edinger (7), born without a neëncephalon, was incapable of the simple functions that a dog deprived of its brain can perform. And apparently many movements that are completely and perfectly executed by the paleëncephalic animal are brought under control of the cortex in the higher animals and man. Thus with greater and greater development in the animal series there is more and more centralization in the neëncephalon. The physical basis for this is apparently the new groups of neurones leading to the cerebrum that are added with greater development. Pike (23, pp. 392, 395) illustrates this as follows:

"It has been shown anatomically and experimentally to some degree, that new groups of nerve cells and fiber tracts appear in the central nervous system as we pass from the lower to the higher vertebrate forms. These new tracts and cell groups lead more and more to the cerebrum or become located in it; hence, the greatest development of the cerebrum and its afferent, efferent and association paths occurs in the human nervous system. As a rather familiar example, the pyramidal tract, arising from cells in the cerebral cortex of the higher vertebrates, may be cited as one of the phylogenetically newer tracts. The frog has no pyramidal tract, but depends upon the rubro-spinal tract as the important part of his motor mechanism.

"In any motor response a great number of afferent impulses of various kinds are involved. It is apparent also that some of these impulses go to the cerebrum almost directly. There is much evidence to show

also that in the higher animals, nearly all other impulses, including those to the cerebellum, get to the cerebrum before they become effective in influencing the motor response."

"Attention shifts therefore, from the afferent pathway to the central system. Somewhere these afferent impulses must be gathered up . . . and passed on to the motor cells directly. The very complexity of the process is sufficient evidence that no one restricted group of cells constituting a hypothetical center could accomplish all of the things to be done. At the present time there is good, and even sufficient, reason for believing that nearly all of these impulses, including those from the cerebellum, pass through the frontal lobes of the cerebrum on their way to the motor neurones. On this view the cerebrum is an essential part of the great motor mechanism. It is in the cerebrum that the summing up or integrating of all the afferent impulses occurs."

Recalling again the reacting function of the nervous system we may note that, among recent writers, Dr. Dearborn (3) maintains that the conception of the reflex arc and of the combination of reflexes give but an inadequate idea of the great complexity of the processes involved, and he thinks that the facts, as we now know them, are better represented by the figure of a circuit and a hierarchy of circuits in which the lower are more or less under the control of the higher. But the facts are even more complex than this.

It is of fundamental importance for the understanding of the genetic point of view, both in neurology and in psychology, to keep in mind the fundamental metabolic and functional processes of the neurones. Not only are the nerve centers and the nervous system as a whole functionally active, their health as well as their efficiency depending upon their functional activity, but even the development of the individual neurone is determined by its metabolism and its functional ability in responding to stimulation. It is well to note briefly here some of the main points in the rather technical contributions made in recent years by the Dutch neurologist Kappers, the Spanish neurologist Cajal, and others.

In 1907 Kappers (1, p. 557) proposed the theory of neurobiotaxis, and in the following year formulated the following laws: 1. If discharge of stimuli occurs in different parts of the nervous substance, then the development of the chief dendrites, and especially the location of the whole body of the ganglion cell, occurs in the direction of the maximal discharge of stimuli.

2. This approximation of dendrites or of cells takes place only between places stimulated simultaneously or in direct succession.

3. The development of the axis cylinder of the so-called central motor system is not conditioned primarily by the motor

function of certain cells, but likewise by the areas stimulated synchronously or successively.

To summarize the law: dendrites and neurites grow in the direction of areas that are stimulated simultaneously with them. This law is in harmony, Kappers maintains, with the psychological law of the association of simultaneous stimuli. Thus he maintains that the fundamental law of psychology is a fundamental law of brain anatomy (1, p. 557).

Kappers has given many illustrations of this law from his studies of phylogeny, illustrations not only for the dendrites, but also for the neurites, and illustrations also of the adjustments or displacement of the cells in connection with this development. Bok (1) in his recent studies has furnished illustrations from the development of the individual. Thus we have now evidence both from phylogeny and ontogeny, which show at least that the processes of the neurones develop especially in the directions of the greatest discharge of stimuli.

Bok has studied the development of the processes in the neurones of chicken embryos, and finds that stimulation is the determining cause of the development of the fibrillae and of the nerve paths. As the result of his studies he has made important corrections to Kappers' law and formulated three general rules in regard to the development of new paths (1, p. 537-539).

1. New paths develop from neuroblasts located near paths that already exist.

2. The young neurone in the beginning has no contact with the path that stimulates it. This is a result of the known fact that the neurite develops sooner than the dendrite. The contact mentioned does not arise until later, by two processes —by the outgrowth of dendrites of the neuroblast and by the formation of collaterals through the path. The activation therefore is in a certain sense a distance-effect, although the distance that is here bridged is a rather small one.

3. The new fibrillae develop in the direction of this stream of stimulation radiating from the path that already exists.

"This coincidence," Bok (1, p. 542) argues, "of the direction of the young fibres, which can be established everywhere, and that of the stimuli coming to them cannot rest on chance, but forces us to consider this stream of stimulation as the cause of the development of the nerve fibres in its direction. The protoplasm located there adapts itself to its function, namely, the conduction of stimuli, by the development of nerve fibrillae. Accordingly this formation of nerve fibrillae and of paths can be referred back to a generally recognized fundamental principle in biology, the adaptation of the protoplasm to repeated function."

Bok formulates his hypothesis of the development of the fibrillae in accordance with this law, as follows (1, p. 542): "Constantly repeated stimuli cause fibrillae to develop along their course."

Every neurite, however, grows with the stream of stimulation, hence away from the cell body, but the dendrite grows in the opposite direction, that is, toward the stream of stimulation. The dendrite is rich in protoplasm, the neurite almost totally free from protoplasm; and we come to the conclusion that protoplasm has the tendency to displace itself in the direction of the stimuli, that is, to seek out the source of stimulation.

Kappers (1, p. 557) showed similarly that the growth of dendrites is determined in the direction of the maximal discharge of stimuli. Thus the ganglion cells in a certain degree are to be compared to the amoeba. The latter sends out its pseudopobia toward the place where the most nourishment is to be found; the nerve cells allow their dendrites to grow in the direction of their functional nutrition, that is, in the direction of their stimulation, and here we can speak of a stimulotaxis just as we do of the chemotaxis in the case of an amoeba.

This displacement of the protoplasm in the direction from which the stimuli come is not always limited to a part of the cell, but can also occur in the whole cell body so that this migrates in the direction of the stream of stimulation in a manner analogous to the movement of the amoeba in the direction of its nutrition. The displacement of the cell therefore occurs in the direction from which the stimuli come, and it is easy to see that thus it occurs in the direction of areas that are simultaneously stimulated, that is, in the direction of the source of the stimuli which stimulate the cells.

Kappers points out that cell-migration in a certain direction is generally preceded by an outgrowth of a large dendrite in which the shifting of the cell takes place. The theory that the dendritic outgrowth follows a preformed plasmodermic path cannot, he maintains, explain the shifting of the cell. His studies, on the other hand, "now covering all the cranial motor nuclei in all classes of vertebrates, have taught him with certainty that for this phenomenon only the process of taxis or tropism exercised by the centres from which the majority of stimuli proceeds to the cell and its dendrites can be considered as responsible." The dendrites therefore demonstrate the law of neurobiotaxis in the manner of their growth.

Kappers (17) in more recent studies has found that the den-

drites are apt to take a shape which gives a maximal amount of surface as a means for enlarging the area for the receptor elements, and he concludes that this surface extension of dendrites of certain cells indicates that here, too, we have to do with a process of neurobiotaxis, a striving for the maximal reception of stimulation in the shortest manner.

Bok (1, p. 561) summarizes his results in substance as follows: If we think of the partially developed nervous system as functioning, the stimuli accumulated in certain paths will radiate from the naked axis fibres into the surrounding protoplasm.

New paths always develop where a diffusion of stimulation of this kind takes place. The young fibrillae develop therefore in the direction of the stimuli, in protoplasm lines which have been marked out by repeated stimulation. They develop hence, from the neuroblast which has been traversed first and is therefore most strongly marked out by paths. This is the basis on which the hypothesis of the generation of fibrillae by stimulation rests.

Repeated streams of stimulation cause fibrillae to develop in their direction. The formation of fibrillae, according to this hypothesis, is in harmony with the general property of protoplasm to adapt itself to a repeated function.

This assumption is supported by the observation that a path at the moment another path appears activates new neuroblast substance along its whole length, from which the neurites again develop in the direction of the stimuli.

Since protoplasm has the tendency to approximate its source of stimulation (positive stimulotaxis), we see that the dendrites are rich in protoplasm, the neurites poor in protoplasm, and that the whole cell body tends to take a position in the direction of the stream of stimuli conducted toward it, that is, according to the law of neurobiotaxis.

This consideration of the history of the development of the system of paths teaches us that the paths do not in the first instance prescribe the way for stimuli, but that it is precisely the stimuli which bring about the configuration of the paths. With the constant action and reaction between structure and function in the central nervous system, function is the primary cause of the arrangement of the anatomical substrate; and thus Bok (1, p. 562) quotes the words of Schiller and says that this gives a deeper meaning to the words of the great German poet: *"Es ist der Geist der sich den Körper bildet"*; "It is the mind which forms the body."

While admitting our ignorance in regard to the poetic interpretation in the closing sentence of Bok's summary, and without attempting to solve that problem of the priority of structure or function, we should note the emphasis upon the influence of function on development brought out by his results.

One is likely at first to fail to recognize the significance of these contributions. If these laws formulated by Kappers and Bok be true, then we have a contribution here of fundamental importance for genetic psychology. Long ago Zanotti and Hume pointed out the fundamental character of the law of association in psychology, comparing it to the law of gravitation in the physical world. Here in the doctrine of the genesis of neural processes and paths we have at least the adumbration of a law equally fundamental for neurology and genetic psychology.

The results of these investigations indicate the neural basis of what Hering (11) long ago called "memory as a universal function of organized matter." This appears as a general property of protoplasm itself, the ability to adapt itself to the functions it performs.

In some such way as the amoeba reacts towards its food, sending out pseudopodia in the direction from which the most nutriment comes, so the protoplasm of the neurone reacts toward the stimuli which affect it, the dendrites growing especially in the direction of the source of stimulation. It is noteworthy that they are the protoplasmic processes of the neurone, and that on the other hand, the neurite or axon, which grows with or in the direction of the stream of stimuli conducted from the cell body, is relatively free from protoplasm.

Just as the reaction of the protoplasm of the amoeba toward its food is called chemotaxis, so the reaction of the protoplasm of the neurone toward the source of stimulation may be called, as suggested by Bok, positive stimulotaxis.

If we add the further evidence of the modification of the neurone by stimulation, from the fact that the cell body itself is sometimes displaced in the direction of the stream of stimulation; that the dendrites show a tendency to spread out in an extended surface in order to give a maximum area for the reception of stimuli; and the fact that the cell body grows with its functional activity, and suffers arrest or atrophies when stimulation ceases; and if we bring into connection with these facts in regard to the development of the neurone the well-known facts in regard to facilitation (2), or what the

Germans call Bahnung, a second application of a given stimulus at a suitable interval having a greater effect than the first, we have an indication of the neural basis of memory and association.

Conclusion

The studies by Parker and Brown suggest the probable genesis of nervous action. Further investigations will probably harmonize these results and give more definite knowledge in regard to the early stages and sequence of development of nervous structure and function. As regards the main point the evidence all points in one direction. If we study the philogenetic series, we find that even before the nervous system appears, reaction to the environment is the significant fact, and that probably the nervous system develops at spots where the primitive animal developed muscle. If we study the individual we find that in the embryo the nervous substance is developed from the outside layer, the epiblast, where the organism comes in contact with the external world. And if we study the individual neurone we find that its development is determined largely by its functional ability to respond to stimulation, its dendrites especially developing in the direction from which the most stimuli are received.

The significant thing is that all of these studies show what has been emphasized over and over again, namely, the significance of function and the fundamentally dynamic character of every neurone. We find here what we find everywhere as we study the physiology and hygiene of the human organism, that function, action, expenditure of energy, as well as the storing up of energy, are the fundamental conditions of life and health. Function is the Alpha and Omega of individual life as well as of the universe in general, a profound illustration of the words of Goethe, "*In der Anfang war der Tat.*"

While the nervous system is, as the older writers already knew, a mechanism for converting stimuli into reactions, our genetic point of view has shown further the two great divisions of stimuli: On the one hand, those that are biologically adequate; on the other, those that act only by association. The former bring about ordinary reflexes and instinctive activities; the latter bring about conditioned reflexes, habits, and the like. The former are mediated by the paleencephalon, the latter by the neëncephalon.

The ability of neurones stimulated simultaneously to affect each other and the ability of the brain cortex to associate

stimuli carried to it simultaneously by different receptor organs, form the neurological basis for mental association, at once the fundamental law of psychology and the foundation for the whole of mental hygiene.

Thus without going into details we can trace the genetic course of development of the nervous system as a reacting mechanism.

1. The nervous system originated in contact with the external world.

2. It originated in spots where the primitive animal had developed muscle.

3. The sequence of the genesis of the reflex arc was: first, the effector organ; second, the receptor organ; third, an adjusting mechanism connecting and correlating the other elements of the reflex arc.

4. There are different forms of nervous action, especially automatic nervous action, reflex action, and conditioned reflex action.

5. The sequence of development seems to have been: first, automatic nervous action; second, reflex action; third, conditioned reflex action.

6. The central nervous system may be conveniently divided into the paleëncephalon and the neëncephalon. The paleëncephalon mediating reflexes and instinctive actions, and the neëncephalon mediating especially association and knowledge.

7. With the higher stages of development there is more and more of centralization in the neëncephalon and more and more of cortical control.

8. The individual neurones develop their processes in the direction of the greatest discharge of stimuli, the dendrites toward the stream of stimuli, the neurites with the stream of stimuli.

9. We find the neurological basis of association in the working out of paths in the nervous substance between areas simultaneously stimulated.

10. Two kinds of stimuli may be advantageously distinguished: first, those that are biologically adequate, or what may be called unconditioned stimuli; second, those that are associated or may be called conditioned stimuli.

Thus we see from the genesis of the nervous system that the one condition necessary for normal development is a rich environment giving plenty of stimulation and freedom for the nervous mechanism to develop in its own way. This seems especially important for the higher parts of the nervous system, the new brain, or the cerebral cortex and its depend-

ent structures. The same thing is emphasized, too, by all those cases of defect where normal stimuli are shut off. In such cases there is always imperfect or arrested development.

The wider relations and wider significance of stimulation and response are suggested by our survey of the development of the nervous system. When we see the rôle of stimulation in the genesis of the nervous system, it need not seem strange that stimulation conditions the normal activity of the organism; conversely, when we see how stimulation is the condition of normal physiological activity, as illustrated so admirably in the physiology of respiration as described by Haldane, it need not surprise us that stimulation conditions the development of the nervous system. No static conception of the human body or of any individual organ is in harmony with modern science or modern philosophy.

Thus the genesis and development of the nervous system gives us the point of view and the method for the study of the hygiene of the brain and mind. This is distinctly in harmony with the point of view and method of the newer dynamic physiology and the newer functional psychology.

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AN ANALYSIS OF A PHASE OF THE PROCESS OF CLASSIFYING[1]

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TABLE OF CONTENTS

I. Introduction

In the course of a recent study of the process of generalizing abstraction,[2] we discovered that our conditions offered exceptional opportunity for an additional investigation of the

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[1] From the Psychological Laboratory of Clark University.

[2] The Process of Generalizing Abstraction; and its Product, the General Concept. *Psychol. Mon.,* 1916, No. 90.

process of classifying. Accordingly, we introduced experiments which were directed to the analysis of this process in one of its phases; and the object of the present paper is to set forth the results of these experiments.[3]

II. Description of the Experiment

A. Problem. Our specific problem was to investigate the mental contents which are involved in the process of classifying, or refusing to classify, novel percepts with a group already familiar. Our approach to the problem has been a genetic one; our aim was to arrange conditions so that the extent of the observer's experience with the group, and hence the possibility of rapid and accurate classification of novel percepts, should increase as the experiments progressed.

B. Method. 1. *Materials and Apparatus.* The material of our experiments consisted in a series of drawings which were analogous to other drawings employed in the experiments on generalizing, and constituting the raw-material of the general concepts formed by the observers in those experiments.

In the experiments on generalizing, we employed four groups of ten drawings each, the drawings of every group containing certain characteristics which were common to the group, and others which were not common. These drawings were of unusual shapes, and they represented no particular objects. With each was exposed a meaningless word which represented the class name of the group, the four words being 'Zalof, 'Deral,' 'Tefoq,' and 'Kareg.' A group of ten drawings (*e. g.,* the Zalof group) was exposed successively to the observer, who was told that he would subsequently be asked to define the group name (*e. g.,* give a definition of 'Zalof'). The successful performance of this task necessitated the observers' looking for the features which were essential to the group. The presentation of the series was repeated at weekly or semi-weekly intervals until the observers ceased to note novel characteristics in the features, when the presentation of another series was commenced. A complete account of the material and method will be found on pp. 34 ff. of our monograph.

The drawings which we employed in our present experiments were capable of arousing novel percepts of a sort comparable with the other homogenous percept groups already experienced by the observer. Of these drawings, some con-

[3] The importance of the investigation of the classification-consciousness has been pointed out by Royce (Recent Logical Inquiries and their Psychological Bearings, *Psychol. Rev.,* 1902, 9, 105-133.) We have found little or no experimental literature bearing directly on the subject of the conscious nature of classifying; this subject has been treated in the literature in close conjunction with the subjects of generalization and the concept. A résumé of the literature of generalization will be found in our monograph on generalizing abstraction (*op. cit.*)

tained the features which were essential to some one of the four generalization-groups, and consquently were homogeneous with this group. Others of these drawings lacked one or more of the essential features, while still others were totally novel. Table I presents (in the first column) the number, (in the second column) the correct classification, and (in the third column) a description of each drawing.

The drawings were made on sheets of four-ply white cardboard similar to those employed for the generalization-drawings. The size, as before, was 20.5 by 12.5 cm. The drawings of the classification-series were exposed singly to the observer (for classification) on a simple tachistoscope, the exposure being made by means of raising or lowering a thin wooden shutter.

2. *Observers.* The observers were five in number. All were trained and skilful introspectors. All excepting one were members of the department of experimental psychology of the University. They were Dr. J. W. Baird, Dr. S. W. Fernberger, Dr. E. O. Finkenbinder, Dr. Miriam van Waters, and Dr. Elizabeth L. Woods.

3. *Procedure.* The experiments were conducted in close conjunction with those on generalizing. After the observer had seen and defined a generalization-group (*e. g.,* Zalof), he was shown a member of the corresponding classification-series, his task being to state whether or not the drawing was to be classified with that series (*e. g.,* "Is this a Zalof?"). He was subsequently asked to furnish an introspective account of his experience in classifying or refusing to classify the stimulus with the group. The time required for making the classification-judgment was taken by counting the ticks of a stop-watch between the time of the raising of the shutter and the response of the observer. This procedure was repeated as often as the time available for the sitting allowed, the number of successive classifications varying between one and six. The total number of classifications and introspections obtained was two hundred and thirty-two.

TABLE I

The cards of the classification-series, with the correct classification and a description of each card. The first column at the left contains the numbers of the cards, the second column contains their correct classifications, and the third column contains brief descriptions of the figures.

Card number	*Correct classification*	*Description*
	Zalof series:	
1	Not a Zalof (?).	Blue central parts; large central body and short protuberances; convex sides; roughly triangular

TABLE I—*Continued*

Card number	Correct classification	Description
2	Zalof..........	Fairly large; very black protuberances
3	Not a Zalof....	Roughly hexagonal in shape; moderate size; no protuberances; center red, roughly five-lobed, minus central circular part
4	Not a Zalof....	Four protuberances; body squarish; body and protuberances black, center dark gray and roundish, minus the three projecting bodies
5	Not a Zalof....	Each pair of terminal arborizations lacks two external fibrils; otherwise figure is Zalof. Red central parts; heavily striated protuberances
6	Not a Zalof....	No protuberances; roughly tre-foil in shape; center blue
7	Zalof..........	Rather small, fine, and light
8	Not a Zalof....	Protuberances not bifurcated, and almost absent, each corner of the figure having four finger-like projections
9	Not a Zalof....	Protuberances arborize wrongly, dividing into two groups of finger-like projections. Center lacks tri-partite form
10	Zalof..........	Protuberances very long, relative to size of central body
11	Not a Zalof....	Protuberances divide into three (instead of two) groups of tentacles
12	Not a Zalof...	Protuberances roughly knobbed at termini, and not arborized; they are relatively very short
13	Not a Zalof....	Central parts missing
14	Zalof..........	Protuberances shortish and roughly striated; triangular form scalene; roughly-formed center
15	Not a Zalof....	Protuberances not arborized, but roughly knobbed
16	Not a Zalof....	Protuberances absent
17	Zalof..........	Smallish; central parts uncolored
18	Not a Zalof....	Terminal arborizations absent
Deral Series:		
1	Not a Deral ...	Left side gray (instead of colored)
2	Deral..........	Vivid red left part
3	Not a Deral....	Lower left straight contour-line missing. Concavity in left contour missing. Leftward point in median line absent
4	Not a Deral....	Right side colored (instead of uncolored)
4a	Not a Deral....	Orientation altered, figure turned through 90 degrees; concavity of right side lacking
5	Deral..........	Right side filled in with large ovalish outline-figures
6	Not a Deral....	Both sides colored. Point in median line faces rightward (instead of leftward). Peripheral contour of each half ovalish, save for basal regions
7	Deral..........	Very small
8	Not a Deral....	Both sides colored
9	Not a Deral....	Point in median line faces rightward. Contour lacks right and left concavities
10	Not a Deral....	Detached rays surround colored (left) side
11	Not a Deral....	Contour on right incorrect

TABLE I—*Continued*

Card number	Correct classification	Description
12	Not a Deral....	Point in median line absent
13	Not a Deral....	Upper lobe of left side absent
14	Not a Deral....	Contour of right side lacks concavity and basal projection
15	Not a Deral....	Contour of left side lacks straight line and concavity
16	Not a Deral....	Extreme lower right peripheral contour of left side not straight, but irregularly convex
17	Not a Deral....	Median line lacks point, but has instead a curve
18	Not a Deral....	Left side filled in with splotches of various colors, instead of having uniform color
19	Deral..........	Right side black
Tefoq Series:		
1	Not a Tefoq (?).	Circular background has irregular patches of color instead of uniform color
2	Not a Tefoq. . .	Angular central part too many-sided; central irregular design incorrect
3	Tefoq.........	Periphery very irregular
4	Not a Tefoq. . .	Indentation in lower periphery missing
5	Not a Tefoq. . .	Periphery perfectly circular; central irregular design absent
6	Tefoq.........	Smallish; background plain
7	Not a Tefoq. . .	Central part curvilinear, instead of rectilinear and angular. Indentation in lower periphery minus one prong
8	Tefoq (?)......	Periphery perfectly circular
9	Not a Tefoq. . .	Central (angular) part uncolored
10	Tefoq.........	Angular central part enormously elongated
11	Tefoq (?)......	Periphery has saw-tooth-like projections
12	Not a Tefoq. . .	Small triangle absent
13	Not a Tefoq. . .	Angular central body turned through 180 degrees
14	Tefoq.........	Small triangle uncolored
Kareg Series:		
1	Kareg.........	Very large; irregular; shaded by lines, slightly tri-dimensional in effect
2	Not a Kareg. . .	Color red; otherwise correct
3	Kareg.........	Smallish; shaded by lines
4	Not a Kareg (?).	Left part divided by vertical axis, and shaded so as to give roughly the effect of two low cones, held base to base and viewed from the side
5	Not a Kareg. . .	Right side and neck missing
6	Kareg.........	Shaded in grays
7	Not a Kareg (?).	Sides outlined by arcs of perfect circles, and shaded with concentric arcs.
8	Not a Kareg (?).	Left side perfect square, shaded to give effect of pyramid with point toward eye

III. Results

A. Introspective Data. In the case of every observer, the examining of the classification figures embraced a peculiar succession of processes which was obviously dependent on

the experimenter's question: "Is this a Zalof (or Deral, etc.)?" This peculiar succession was characterized by the fact that the attention and regard moved in a more or less rapid course over the regions of the figure which corresponded to those regions of the generalization-group that had been found to contain essential features. Moreover, the behavior of these regions themselves in consciousness varied in a manner which was correlative with their objective resemblance or lack of resemblance to their counterparts in the original group. While the observers thus showed a remarkable uniformity in the general course of their consciousness, they nevertheless differed in many respects, both structurally and functionally. Structurally, the variations consisted in the nature of the imagery which supplemented the observers' classifyings of the figures.[4] Functionally, the variations were concerned with the extent to which supplementary imagery appeared, with the conditions under which it occurred, with the extent to which the course of attention was deflected to the regions of striking variables, and with the relative strength or dominating power of the main coursing of consciousness over the figure (which constituted the process of classification) on the one hand, and of the behavior in consciousness of separate features, on the other. We shall begin by presenting a number of typical introspections of each observer's classifications of the figures; and we shall then present a general description of the processes which constituted the classifications, endeavoring to indicate both the similarities and the differences among our observers. With each introspection we shall include a mention of the time which elapsed between the presentation of the card, and the verbalizing of the judgment.

Observer A

Zalof, Card 3 (November 18, 1912) "No." Time, .5 sec. Introspection: "I simply noted the shape of the figure,—which was round and large. I had no attitude of looking further, but immediately responded 'no.'"

Zalof, Card 5 (December 3, 1912) "Yes." Time, 1.5 sec. Introspection: "My glance went first to the branch on the right; I saw the ends of the branch, and then the center, and started to say 'yes.' My 'yes' almost stopped, when I noted the presence of four branches; at this moment I had a visual image of the original three branches in the figure."

Zalof, Card 7 (December 3, 1912) "Yes." Time, .9 sec. Introspection: "The features of the figure came into my attention very

[4] We shall use the term 'classifying' (unless otherwise qualified) in referring to the total observation of the classification-figure, together with the affirmative or negative judgment in which it culminated.

quickly, one after the other. Immediately I said 'yes.' After that I was aware of a tendency to verify my judgment; this consisted in the appearing of a visual image of a figure of the original series which resembled the figure which had just been exposed, and in my viewing the two images side by side."

Zalof, Card 10 (December 3, 1912) "Yes." Time, 1.75 sec. Introspection: "First of all I was vaguely aware of the whole figure; then I ran my eyes out to the ends of the various branches. The sub-branches terminated in points, and that seemed to 'stir up' my answer 'yes.' As soon as I had given this affirmative response, I was aware of a desire to look at the central parts of the figure,—to see if that center were present. This consisted mainly in a turning of my attention toward that region, together with visual imagery of what the center might have been. I was also aware of a visual image of the first member of the Zalof series, in which the center was clearer than the rest of the figure."

Zalof, Card 11 (January 14, 1913) "Yes." Time, 1.2 sec. Introspection: "I was aware, at the outset, of looking fixedly at the center; my attention was held by the various lines about the center, and this amounted almost to a confusion or a distraction. As soon as I could, I removed my attention from the central region and my glance went to the ends of the branches. I was aware of two groups of branches at the end of each main branch,—I glanced in turn at the ends of each of the three branches,—and then I said 'yes.' The general shape and the presence of the two groups of branches at the end of each main branch meant 'yes,'—meant Zalof. My actual perception of these features gave me my affirmative response."

Zalof, Card 5 (January 28, 1913) "No." Time, 4.2 sec. Introspection: "My eye fell first upon the right-hand part of the figure and I was vaguely conscious of its blackness, although I did not attend to this. Instead my attention immediately went to the essentials of the figure; and I noted that the branch which I was fixating had two groups of tentacles at its end, and that each of these two groups was divided into still smaller ones. I found, however, that these smallest branches were only two in number. I glanced toward the left, and saw that the end of the next branch was the same. I then glanced at the lower branch and found that this also was the same. The presence of these variations in the ends of the branches compelled me to say 'no;' this compulsion consisted in the fact that I tended to turn away from them as from a difference. Then the presence of the black lines extending through the tentacles stood out; this constituted another prominent difference and seemed to add to my tendency to turn away from the figure. The whole situation meant 'no.' I did not take time to observe any other features, but immediately gave my negative response."

Deral, Card 3 (December 10, 1912) "No." Time, 2.8 sec. Introspection: "I was aware of the shape of the present figure; it had two parts, one at the left and one at the right. I called up images of the original figures, which I compared with the percept; I looked at several of them to see if they had this particular shape and size. At that time I was wavering between tendencies to say 'yes' and 'no;' I might have said either. I was aware of hesitation; I was then aware that this percept could be called a Deral, if it were cut in a certain way; I had imagery of the experimenter in the act of cutting it in this fashion. At the same time, I was aware that the images of the original figures did not in any case possess the same

shape as my percept; this fact was the clearest in my consciousness. My answer 'no' followed."

Deral, Card 2 (January 21, 1913) "Yes." Time, 3.4 sec. Introspection: "I had to investigate every part of the figure, because it was so extremely red. My attention went to the redness; I then began investigating the other parts,—the little angle at the right-hand side, the projecting dotted area, and the rest. The little angle of the base of the left-hand side stood out clearly from the straight line at the base of the central projecting part of the figure (right-hand side). The dotted area was remarkably large. In the case of every feature I was aware first of a feeling of familiarity with that feature, and next of a verification of it by comparing it with images of the original series. My affirmative response followed."

Deral, Card 4 (January 28, 1913) "Ye-e-e-s. The color upon the other side is just a deception; the form is the main thing." Time, 1.8 sec. Introspection: "My attention went first to the form. When I perceived its doubleness I was aware of clear visual images of some of the other figures, which I compared with the present one. I knew that they were not the same in all respects, but that they were the same in the essentials. The 'yes' started to say itself and then I perceived the color. My awareness of the color tended to check my 'yes' but it did not constitute enough of a check to make me say 'no.'"

Deral, Card 5 (February 4, 1913) "Yes." Time, 2.2 sec. Introspection: "I attended to this figure in the same way as I had attended to the figures of the original series, in their previous exposure. My attention went first to the two straight lines; these stood out easily and quickly, and no differences occurred as my attention passed across them. The way I perceived the lines,—the ease and quickness with which my regard went over them,—the fact that they demanded no attention—meant 'same.' I glanced at the rest of the figure, seeing the lobes and the central projecting part; my response came automatically; and at the same time, or immediately before, I saw the lines in the figure. These persisted most prominently after the response."

Tefoq, Card 1 (February 18, 1913) "Yes." Time, 1.8 sec. Introspection: "The figure was vague at first; presently my attention went in a half-hearted fashion to the central region, although I did not see this in detail. It then went to the indentation in the lower periphery. I then saw the colored patches; my awareness of this was followed by vocal-motor imagery of saying 'no.' I then turned to these essential things, and made an effort to recall the figures of the original series. I saw that certain features were common; but I was unable to recall another case in which the colored patches on the background were present. These were not essential, however, and I answered 'yes.'"

Tefoq, Card 4 (March 11, 1913) "No." Time, 2 sec. Introspection: "During the first moment the perception cleared up in my consciousness. I then found that the main features of the central region were present; they stood out easily in my attention. I especially noted the little blue object sticking up behind the central region ['blue' triangle]. I then noted the absence of the 'Chesapeake Bay' indentation, and automatically I said 'no.'"

Tefoq, Card 11 (March 18, 1913) "No." Time, 1.6 sec. Introspection: "The periphery of the figure,—the saw-teeth,—stood out

clearly in my attention, and also the plainness and whiteness of the background surface. It never stood out very clearly; I started to say 'no' before it had fully cleared up. I can not recall what the central region was like; I saw it only in a brief glance. I did not look for the 'Chesapeake Bay' indentation."

Tefoq, Card 13 (March 25, 1913) "No." Time, 7.2 sec. Introspection: "At the very outset, I knew that the central part was wrong, but I could not tell what was wrong with it. The progress of my attention was blocked by it; I could not get beyond it, and I examined it part by part, beginning with the decoration upon the end which faced me. This possessed the points which I had observed in the original figures. My awareness of this was almost in the direct perception itself; but there was some comparison of it with schematic visual imagery of this decoration as it occurred in earlier figures. I noted next that the central part possessed depth, as had the central parts of the original series. I began an effort to call up the original figures; I could not recall all of them, and their presence did not seem to aid me. Very suddenly I discovered what the trouble was: I saw the central part inverted; I realized that the wrong end of the 'piano' was facing me. Just at this moment, I perceived that the triangular patch did not stick up from behind the central figure. My awareness that I was looking at the wrong end of the 'piano'* consisted in suddenly seeing the 'piano' turned about, nothing else in the figure being altered. Immediately my negative response followed."

Kareg, Card 1 (April 1, 1913) "Yes." Time, 1 sec. Introspection: "I observed the main outline, which seemed the same as the Karegs, and the fact that it possessed depth; my attention was particularly upon the lines which shaded the figure. My 'yes' followed immediately. After the exposure was terminated, my awareness of the lines continued in visual imagery; there was a vague unfamiliarity about them. The ease and rapidity with which my attention passed over the main form was the sameness; I did not call up a single visual image. The lines were a disturbing element."

Kareg, Card 4 (April 1, 1913) "No." Time, 1.2 sec. Introspection: "The figure lacked familiarity from the outset. It did not look like the other figures. This was present in the hesitancy with which I regarded the form and shape of the figure. While regarding this, I said 'no;' afterwards I was aware of imagery of the figures of the original series, but none of them possessed such an outline."

Kareg, Card 5 (April 1, 1913) "Ye—No." Time, 1 sec. Introspection: "At the very outset, I was aware of the shading and the sides of the figure. This stood out easily and I started to say 'yes.' Then I observed that no smaller right-hand part was present and I said 'no.'"

Kareg, Card 8 (April 15, 1913) "No." Time, 1 sec. Introspection: "First of all I perceived the angles at the top and at the bottom; I observed that the outline was cube-like, with one point facing me. My reaction 'no' followed immediately."

OBSERVER B

Zalof, Card 1 (November 18, 1912) "Yes." Time, 1 sec. Introspection: "At the outset, my attention fastened quite involuntarily

* *A* had previously likened this central part to an upright piano.

upon the center. I noted the presence of four component forms, my idea of fourness being present in four definite movements of my eyes. I next looked at the three branches of the figure, and noted the bifurcation at the ends, whereupon I said 'yes.' I then observed attentively the blueness of the center; immediately I was aware of a slight catch of my breath, which functioned as a questioning,—'must the Zalofs be red in the center?' I was then aware of a definite visual image of a Zalof of the original series, whose tentacles were similar to those of the card which I had just seen."

Zalof, Card 2 (November 18, 1912) "That is a Zalof." Time, 1.25 sec. Introspection: "My eye fastened upon the center first; then I noted the bifurcations at the ends of the tentacles, and then the tentacles themselves. The color did not reach focal attention until later. The whole gave me an impression of bigness, present in the extensity of my eye-movements themselves. I was aware of a tendency in the muscles of the left hand to twitch, as if to react by tapping; I then gave my verbal reactions. Apparently I did not have the proper reaction mechanism at hand."

Zalof, Card 3 (November 18, 1912) "No." Time, 2.5 sec. Introspection: "At the very outset I perceived that this was a pentagonal figure. My reaction 'no' followed immediately, and simultaneously my attention went to the center and I noticed that there were three main parts of the central body, but also three smaller ones. At the same time I noted the color of the center; this seemed to bring in a visual image of a member of the series, but one with distinct projections."

Zalof, Card 4 (November 18, 1912) "No." .75 sec. Introspection: "The fourness of this figure flooded in upon my attention; it came to consciousness in terms of oculo-motor innervations. I then noted that the branches terminated in bifurcations, and this was followed by a peculiar consciousness, which meant 'not on your life; you won't fool me.' This consciousness is hard to describe; it came out in my belligerent 'no.' Before I reacted I attempted to fixate upon the center; I got only a shadowy dim gray of a circular outline, but no clear impression."

Zalof, Card 6 (November 25, 1912) "That is not a Zalof." Introspection: "My attention went immediately to the inner parts, the outline being less clear. On the basis of the form of these parts I was aware of a tendency to say 'yes.' Their blueness did not seem to interfere with my affirmative tendency. My attention then went to the outline,—to the fact that the corners were smooth. A visual image of an arborized tentacle came up, with auditory imagery of my own voice saying the word 'bifurcation.' Then I was aware of vocal-motor verbal imagery of 'no Zalof,—has rounded corners' and I gave my reaction." (Describe your first affirmative tendency.) "This was partly a muscular innervation in my hand for tapping and partly a vocal-motor verbal 'yes' or 'that's a Zalof.'"

Zalof, Card 9 (December 5, 1912) "That is decidedly not a Zalof." Time, 1.9 sec. Introspection: "My attention went first to the general form, and to the fact that it possessed curves which had not been present in the Zalofs. It next went to the tentacles and I perceived that there was a bifurcation at the end of each tentacle. All of this time I was aware of a growing 'yes'-consciousness; this seemed to consist in a relaxation of my attention, having something to do with eye-movement. It was bound up with change in direction of atten-

tion and relaxation of strain. I then turned my attention in a rather voluntary fashion to the center. Immediately 'no' appeared to my consciousness in auditory and vocal-motor terms. My reaction followed."

Zalof, Card 9 (December 10, 1912) "That's no Zalof." Time, 1.5 sec. Introspection: "My attention went first to the center; and this was not right. I did not react immediately, but waited until my eyes had wandered over the periphery; the periphery was not satisfactory, although I was aware that the tentacles had a slight tendency towards bifurcation. My decision was made as soon as I obtained a clear perception of that center which lacked the three ovular bodies. It was present in a tendency to turn away from the figure and to react; and I think I actually turned my head and body slightly. My awareness that the periphery was not satisfactory consisted in a dim visual image of the form of bifurcation which is characteristic of the original series; it seemed to come to my consciousness along with the visual perception of the tentacle. I was aware of a peculiar attitude of hostility; the Zalofs seem to be good to look at, and the other figures are to be cast out. This attitude was present only in the affective toning with which I threw it out."

Zalof, Card 10 (December 10, 1912) "Yes." Time, 3.2 sec. Introspection: "My attention fastened upon the center, where it remained; the central bodies were not clearly marked out. The figure was so small that I could not see whether there were separate central bodies. After a time I turned to the periphery; I was fairly certain that it was correct, but I investigated each tentacle in turn. My certainty was present in the relief from tension in my chest, shoulders and eyes with which I turned to successive tentacles, and finally back to the center. I was unable to detect more clearly the arrangement of the central parts. I was aware of a 'yes' consciousness,—auditory and vocal-motor of the word 'yes.' I then noted the very decided curve in each of the tentacles and was immediately aware of a visual image of the curve in the tentacles of the first Zalof of the series. The whole process presently dropped out of mind, and that meant that this characteristic was all right. Finally I was aware of a visual image of the tiny Zalof of the series, in which I had been unable to perceive four separate bodies in the center. Thereupon I gave my answer."

Zalof, Card 12 (January 14, 1913) "No." Time, 1.6 sec. Introspection: "Attention at the outset fastened upon the center. I examined it closely; I found that there were not four bodies in the center. My reaction followed immediately. As the curtain was falling the word 'smooth' came to my mind with a tendency to turn toward the corners of the figure; I can not remember now how these were formed, but I am certain that they were smooth and not bifurcated."

Zalof, Card 13 (January 14, 1913) "No, *ma'am*." Time, 2.1 sec. Introspection: "The figure cleared up in my attention,—all black. My fixation seemed to go out to the periphery; then I had to will my attention back to the center. This was very different from the others. I became confused; I had a peculiar bodily feeling as though I were suspended in the air. For a moment I lost my *Aufgabe*,—I forgot what I was to do with the figure. Then I think that I superimposed a visual image of a correct central form (uncolored) over the center of the percept; my knowledge that this was nothing like the Zalof came out in my 'no, ma'am.'"

Zalof, Card 2 (January 21, 1913) "Yes." Time, 2.2 sec. Introspection: "My first reaction was negative; the blackness and the heaviness of the figure compelled my attention, striking me with a strange new feeling. This strangeness is difficult to describe; it involved a shallower breathing, a fixating, and a kind of a surprised 'no' in vocal-motor imagery, with an explosive innervation back of it. From this time on my attention traveled about the figure; I noted the general triangular form and the tentacles; I examined the center carefully and found four red bodies. Still my attention tended to fasten upon this heavy shading at the terminations and the feeling of strangeness persisted. Visual images of figures of the original series which contained decided variations in the shading came to my consciousness; at this point I was aware of a great deal of satisfaction and of a release of my attention from the heavy shading of the figure. My reaction followed."

Zalof, Card 15 (January 21, 1913) "No, sir." Time, 3.2 sec. Introspection: "My attention went to the general form of the figure,—the triangular body, which agreed with the figures of the series. Then my attention went to the central parts, which were properly composed of four bodies. Then my eye traveled along the periphery and I noticed the ends of the tentacles, which were entirely wrong; I realized this before any visual image of other bifurcations appeared. I was aware of auditory imagery of the word 'bifurcations' and of a sort of disgust; I then verified and compared my percept with visual imagery of one of the larger figures of the original series. My reaction followed." (Describe your consciousness that the triangular body agreed with the members of the original series.) "I was aware of imagery,—visual and other,—of being seated at the other apparatus and being shown the first member of the original series. The bodies were similar in the image and in the percept; my consciousness did not remain upon this feature, but floated smoothly to the next figure."

Zalof, Card 17 (January 27, 1913) "No." Time, 2.2 sec. Introspection: "My eye travelled over the contour of the figure, and it was correct; I noted the bifurcations, and the fact that my eye passed on without stopping meant that the form was correct. I was aware of pleasantness, and of a tendency to react with 'yes.' Then I looked at the center; the form of the central bodies was perfect. Then the word 'red' appeared, with questioning inflection, and my reaction was 'no.' Immediately several images of Zalofs in which the center was invariably red flooded into my consciousness."

Zalof, Card 5 (January 27, 1913) "Ye-e-s." Time, 3 sec. Introspection: "The bigness, and the coarseness of the figure seemed to give me a negative tendency. I was aware of verbal imagery of 'no.' Then I began a detailed investigation of the figure; my eyes went to the center, which was approximately correct, and then to the periphery. The periphery was right; my awareness of this came in terms of auditory imagery of 'bifur—,' the word remaining incomplete. Then my attention returned to the center and I looked at the three bodies which project in the direction of the tentacles; I could make these out although they were not very well differentiated. The middle body was there, and also not well differentiated. The color was proper. I gave a hesitating reaction."

Zalof, Card 17 (January 20, 1913) "It is. Oh, no! It isn't! It isn't!" Time, 11 sec. Introspection: "I became aware of the form on the first glance. It was pleasing. I glanced at the center; this

seemed to satisfy me, as my attention passed readily away. My glance went out to the tentacles; they were correctly formed. I reacted instantly. Then came a visual image of the figure, and I realized that the center was not red but black. My attention for the first time semed to focus upon the center, and the word 'red' rushed in. My negative reaction followed immediately. Afterwards I was aware of visual memory-images of Zalofs with red centers, and of auditory imagery of having described the Zalof as always having red centers."

Zalof, Card 18 (February 6, 1913) "No." Time, .8 sec. Introspection: "My eye fell first upon the periphery; the tentacles had no arborized ends. My decision was ready at this instant; yet I was aware of a sort of inhibition which seemed to consist in a mechanical carrying out of my scheme of observation. My eye traveled to the center before I reacted. Immediately after my negative reaction, or with it, I was aware of a visual image of bifurcated ends which seemed to be superimposed upon the percept. The smoothness of the ends seemed to be a positive thing which simply entered consciousness and set off my reaction almost reflexly."

Tefoq, Card 5 (January 27, 1913) "No." Time, 1.4 sec. Introspection: "My attention went in turn to the periphery, to the blankness of the circular background and to the end of the central part. I was aware of visual imagery of the irregular leaf-like form of the periphery in one of the original Tefoqs. My attention riveted upon the white blank end of the central 'steps' [the angular central figure] and my 'no' followed immediately. I think that 'hi—,'—the first of the word 'hieroglyphic' [B's name for the little irregular design in the center of the angular central figure] came to consciousness in auditory imagery, or in a setting of my mouth for pronouncing the sound; this was very vague and non-focal."

Tefoq, Card 1 (February 3, 1913) "Yes." Time, 5 sec. Introspection: "My attention went first to the stair-step central figure. This was all right. My regard then passed to the indentation in the lower periphery and followed around the periphery. I then noticed the design in the end of the central part and this was likewise correct. I was aware of a tendency to react, which was immediately followed by focal attention upon the figures on the background part. This attention was accompanied by a parting of my lips, an in-take of breath, and a leaning toward the card. Presently I was aware of a visual image of a member of the original series, in which much shading was present in the background part of the figure. Then I turned to my essential features once more, and went over them three or four times, noticing the design on the end of the central figure, the periphery, etc. All of this time the figures on the background of the percept were prominent in my consciousness. Finally I dragged myself to react."

Tefoq, Card 3 (February 3, 1913) "Yes." Time, 2.2 sec. Introspection: "My eye first fell upon the form of the central part of the figure. The word 'green' came to mind. My attention passed easily from this central region, and I noticed the blue triangle, from which my attention again shifted easily. I observed the design in the end of the central part and said 'yes.' The outline was in the fringe of my consciousness throughout."

Tefoq, Card 4 (February 10, 1913) "Yes." *No!!*" Time, 2.2 sec. Introspection: "My attention passed over the central part of the figure; during its course, the following verbal imagery appeared, in

vocal-motor terms: 'steps washed in green;' 'triangle blue;' 'design,—' then 'yes.' Afterwards I was aware of a visual image of a figure, in which my attention was upon the lower outline and upon the absence of the indentation. Immediately my 'no!!' followed. Before or during this 'no!!' I had very good visual imagery of several indentations in their proper form."

Tefoq, Card 6 (February 17, 1913) "Yes, that is a nice little Tefoq." Time, 1.8 sec. Introspection: "I noticed at the outset the relative smallness of the stair-step form and the bigness and blackness of the outline. This was followed by a rigidity of body which constituted a somewhat negative reaction. My attention then went to outer parts of the figure, and I was aware that these corresponded to my definition of what the outline should be. The word 'inset' occurred in auditory imagery of my own voice, and I was satisfied as far as the periphery was concerned. I was then aware of auditory and vocal-motor imagery of 'green wash' and I noted its presence. I then noted the tiny blue triangle and the design in the end of the central part. I drew a little closer and was aware of pleasantness, just before I voiced my decision."

Tefoq, Card 7 (February 17, 1913) "No." Time, 1.4 sec. Introspection: "At first my attention went to the inset in the lower periphery, then to the green wash on the side of the central body and then to the bright blue triangle. I was aware of an affirmative tendency, and almost had vocal-motor imagery of 'yes.' Then my attention fell upon the curve, where there should have been sharp corners. I was aware of a decided shock, and of kinaesthesis of closer scrutiny on this feature. Then I reacted with 'no' before I observed whether or not the design was present in the end of the central figure. I think there was no design."

Tefoq, Card 10 (February 17, 1913) "Yes." Time, 3.6 sec. Introspection: "At first I was aware of extreme interest in the figure. I glanced it over quickly, and from this moment there was a consciousness that in spite of the strangeness of the figure, it would turn out to be all right. This consciousness, I think, consisted in a quick grasping of all of the essential features. Then I began deliberately noting each detail; first of all the outline. Before my attention went to the other details I had a visual image of one of the Tefoqs, in which the central part extends beyond the right periphery; and also auditory recall imagery of having mentioned to you that the outline need not include the whole of the step. The auditory imagery was not clear and detailed. Then my attention went to the 'step' (central figure) itself. I noted that the form and color was right. 'Green' was present in auditory and vocal-motor terms. I noticed the outline and the design in the end, and then came my reaction."

Tefoq, Card 11 (February 24, 1913) "Yes." Time, 2.6 sec. Introspection: "At the start I was aware of dissatisfaction. I could not remove my attention from the periphery; it remained there, and at the time I was aware of a certain amount of tension in my chest, eyes and forehead. I seemed to drag my focus away from the periphery by main force. I then turned to the central figure and began going over my criteria,—the greenness, the blue triangle, the design in the end of the steps. Then my 'yes' followed."

Deral, Card 1 (March 3, 1913) "No." Time, 1.9 sec. Introspection: "My attention went over the entire form of the figure and the foot on the right-hand side and the humps; at the same time I was

aware of a visual image of the first Deral which had about the same size and form as the present figure. The word 'fish' was also present. What was most focal in my mind at first was the visual perception of the form, the immediate and ready slide of the attention from one figure to the other, and the absence of a tendency to hesitate or to make comparisons. At the close of this survey I started to say 'ye—,' that being present in vocal-motor imagery. Then my eyes went to the left-hand figure, and the word 'black' appeared; hesitation followed, and then the words 'not colored,' and then visual imagery of colored figures."

Deral, Card 2 (March 13, 1913) "Mm—hm."[5] Time, 3 sec. Introspection: "My first tendency was negative. The 'Mm—hm' started to be 'Mm—mm.' At this time my attention was on the red color; and clung tenaciously to that color. Instead of giving the negative reaction, I busied myself with the other features; I found myself going over the figure very carefully. I first noticed that the right-hand figure had cilia. Then I attended to the outline of the right-hand figure and I was conscious that the cilia ran all about the periphery. I noted the smoothness of the left-hand figure; then my eye passed downward to the bottom of the left-hand figure and I noted that it stood on a triangle. At this time I was aware of auditory imagery of 'triangle at the bottom.' The bright red coloring was clear in my consciousness. My attention then went again to the form of the right-hand figure, in order to note whether the right-hand side had a bowed form. I found that it did. Still, my affirmative reaction was inhibited. The figures did not seem to stand at the same angle as the rest of the figures. This occupied my attention for a perceptible length of time. Finally I reacted affirmatively." (Describe your statement 'in order to note whether the right-hand side had a bowed form.') "My observation of the outline of the right-hand figure was initiated by a visual image of a member of the series in which that outline was bowed."

Observer C

Zalof, Card 1 (November 23, 1912) "No." Introspection: "My attention went to the triangular form and the three branching-off processes with the fibroid processes at the ends. When I looked at the blue color, I was aware of some hesitation regarding my own definition and of wondering whether color should have been made an essential element. My observation of the triangular arrangement was attended by a vague visual image, the form of which was defined not by lines, but by something kinaesthetic, of the nature of empathy; I was aware of a feeling of being stretched out like a starfish; this consisted in a slight pull of my shoulder muscles and a feeling of the center being located in my chest. It was very fleeting. My wondering whether the definition should have made color an essential element was for the most part present in vocal-motor verbal terms, to the effect that the definition might well include only the triangular arrangement and the branching processes, and leave out the matter of color. I had a vocal-motor tendency to use the phrase 'too obvious,' meaning that my definition was too obvious when based on color. This consciousness was not definitely evolved at the time; it was simply 'too obvious.'" (What was the immediate antecedent of your reaction 'no?') "It was focussing on the part of the figure

[5] 'Mm—hm' is a colloquialism of affirmation, 'mm—mm,' of negation.

colored blue. From the very first I had been aware of a negative attitude; it was rather definitely localized in the muscles between my eyes, as if I were squinting and wrinkling my forehead. There was also a slight unpleasantness, although 'strain' expresses the consciousness better. Then came the consciousness that I would like to revise my definition."

Zalof, Card 2 (November 23, 1912) "Yes." Introspection: "I was aware of a fragmentary and unclear empathic kinaesthesis or attitude, similar to the one which I described in connection with the first card (card 1). The vocal-motor 'yes' came before I spoke, and it followed very closely upon the kinaesthesis."

Zalof, Card 3 (November 23, 1912) "No." Introspection: "The processes were very fleeting. I had a tendency to say 'no,'—strains in my throat muscles,—immediately after you exposed the figure. Then came a sort of internal 'why,' partly vocal-motor. Then voluntarily my attention moved around the figure, following its outline; there was a feeling of no angles, present particularly in vocal-motor terms of 'angles' and partly in a kinaesthesis of eye-movement. At the end of this survey of the thing, my reaction 'no' occurred. The first 'no' was much more vivid and intensive than my finally-spoken 'no.'

Zalof, Card 4 (November 23, 1912) "No." Introspection: "I was aware at the outset of a visual image of the words 'no red,' in my own handwriting, held off at a distance; and also of a kinaesthetic pull over to the left. My feeling of empathy recurred, but its balance was disturbed by a pull to the left. Then, with voluntary attention, I fastened my eyes upon the center of the figure; then came a vocal-motor tendency,—'that bally definition.' I was aware of unpleasantness; and a vocal-motor 'no' grew up slowly, beginning with a sensation of pressure at the lips and with a slight bending over toward the right side. Then came a kinaesthesis at the back of the tongue, which held for an appreciable period of time. Then 'no' was pronounced slowly. This feeling of disturbed balance and unpleasantness persisted throughout; when I gave my reaction it disappeared."

Zalof, Card 6 (December 6, 1912) "No." Time, 1 sec. Introspection: "My attention went to the general form of the figure, and the following vocal-motor verbal process went on: 'general form has no distinct off-shoots; points are not equidistant from center; color blue; background covered with dots, no repetition, in center, of general pattern of off-shoots from periphery.' This vocal-motor process was to some extent innervation,—'blue' came out,—but it was mostly a feeling of strain and slight tongue movements. I was also aware of kinaesthesis in my left hand of pointing to the dots; they impressed me as being new, and their newness consisted in their visual clarity itself and in this kinaesthesis."

Zalof, Card 7 (December 6, 1912) "Yes." Time, .9 sec. Introspection: "I was aware at first of a visual image of a larger pattern of this same stimulus, and my affirmative reaction came immediately after this image; it appeared first in the form of a slight bending forward at the back of my neck, and the vocal-motor 'yes' came immediately. After I had pronounced the 'yes' I was conscious of the fact that my attention to the stimulus had been concentrated entirely upon the periphery of the design. This was known in terms of a recurrence of kinaesthesis of eye-movement which had been

present during my observation of the figure; a vocal-motor strain,—'why didn't you look in the center?' appeared, together with a visual image of a large, thickened center formation. Afterwards came a visual image of the center of the stimulus, which was merely red; all I could get was the color, and not the form. Along with it came a kinaesthesis of fixing my eyes upon the center, and a faint unpleasantness."

Zalof, Card 8 (January 31, 1913) "Yes." Time, 1.3 sec. Introspection: "I observed in turn the outlines of the central part, the red color, and the three main arms. With this came a kinaesthetic feeling of assent in the back of my neck,—a kinaesthetic image of nodding."

Zalof, Card 9 (January 31, 1913) "No." Time, 1.2 sec. Introspection: "My attention went to the lumpy, irregular outline, the blueness, and the ends of the tentacles. With my attention to the form of the ends of the tentacles, my negative response began, as simply the closeness of my attention. Just as I said 'no' I was aware of a definite visual image of a large, very simple Zalof."

Zalof, Card 11 (February 18, 1913) "Yes." Time, .8 sec. Introspection: "I first focused upon the center of the card. There was a quick upward eye-movement, and vocal-motor imagery of 'two arms.' Immediately the reaction followed."

Zalof, Card 13 (February 18, 1913) Shakes head. Time, 1.4 sec. Introspection: "At the very outset I was aware of a vocal-motor tendency to react 'yes,' followed by an inhibition which took the form of a tongue movement; at this time I had a feeling that something was absent, and then the reaction 'no' appeared in vocal-motor terms. I gave the reaction, and then came a vocal-motor verbal image of 'red' and a visual image of a red splotch with a vocal-motor 'Zalof.' My feeling of something absent seemed to be a concentration upon the center of the figure."

Zalof, Card 5 (February 25, 1913) "Yes." Time, .8 sec. Introspection: "My attention focused upon the upper right-hand arm, the rest of the figure being present in peripheral vision. 'Yes' came up in vocal-motor imagery, and my reaction followed immediately."

Kareg, Card 2 (March 11, 1913) "No! Mercy, that's funny!" Time, 3 sec. Introspection: "At the very first I was aware of a strong tendency to react 'yes,' present in considerable innervation, and moving of the tongue. My attention then centered upon the connecting structure, and I was aware of a strong kinaesthesis in my neck of having my head bent in the position of the figure. This was unpleasant. At the same time I was aware of a visual image of a clam with its protruding neck bent down. Just before I reacted I had a visual image of the printed words 'in spite of' and some awareness of 'in spite of stronge resemblance.' The words 'strong resemblance' were present in faint vocal-motor terms. My reaction followed. The head of the figure ought to go out straight instead of being bent down in a curved line as it was in this figure."

Kareg, Card 8 (March 25, 1913) "No." Time, 2.4 sec. Introspection: "I noted the form of the figure, with the awareness that the large body was on the right-hand side. I was also aware of a visual image of the turtle-like Kareg [of the original series], the large portion of the figure being on the left; at the same time came the vocal-motor imagery of 'they are all like that.' My eyes were focused on the figure on the left where they tended to remain. A

vocal-motor strain for saying 'no' appeared and then the spoken reaction itself."

Deral, Card 1 (April 15, 1913) "Yes." Time, .8 sec. Introspection: "My attention went to the lower part of the right-hand figure, and then the line of regard moved upwards obliquely in a broad bend to the upper left-hand part of the figure. At the angle in the line between the left and right-hand figures I was aware of a slight kinaesthesis of a hitch or jerk, and then of satisfaction. This constituted a consciousness that my oblique line of regard had taken in all of the angles which I had discovered in the figure. (At present I am not at all sure that it did.) My reaction followed immediately."

Deral, Card 4 (April 15, 1913) "No." Time, 4.2 sec. "As I observed this figure, I was aware that it was tilted, that it did not stand square. I had a kinaesthesis of whirling and dizziness and of decided tilting of my whole body, of my head being nearer the ground than normal. The kinaesthesis came towards the last of my observation. My negative reaction followed."

Deral, Card 8 (April 15, 1913) "Ye-e-es; I don't know." Time, 1.2 sec. Introspection: "My first perception of the figure was followed by a feeling of balance, a consciousness that the figure had the proper attitude. This kinaesthesis of balance was strongest in my right hand, and it consisted in a sense or awareness of taking hold of the figure and settling it squarely, in an upright position. Immediately I said 'yes;' afterwards I was aware of a lingering focus upon the upper right-hand part of the figure which seemed to have a finger-like structure not present in the Derals. I dwelt upon color in my primary memory image of the figure; I was aware of hesitation and of a vocal-motor image of 'color?' with decided questioning inflection."

Deral, Card 1 (April 29, 1913) "Yes." Time, 12 sec. Introspection: "At the outset I was aware of a strong tendency to react negatively. This came as a vocal-motor image of 'no' and a kinaesthetic tendency to focus upon the uncolored left-hand side of the figure. Then came a kind of focusing both of my eyes and of the direction of my head upon the lower part of the figure. A visual image of one of the Derals which I had just been shown came up rather clearly; I was aware that there was no color in my percept and that certain typical angles were present in both percept and image. [During the previous exposure of the series, I had had a vocal-motor experience of dividing the Deral into two types, as regards the angles; in one type the acute angle is very acute, and is lower in the figure; in others, it is less oblique, and further up in the figures.] A vocal-motor image of 'acute angle' now occurred, and a wave of recognition which consisted in a strong affective toning, —pleasure, with a strong gasp outward,—and considerable empathy of sharp bending or contraction. Then came a definite affirmative kinaesthesis in my head, of nodding, and in my hand of a waving gesture. The vocal-motor verbal image of 'yes' appeared. Then came the reaction; afterwards I was aware of strong negative and strong affirmative tendencies which seemed to project themselves respectively into the two sides of the figure. I had a visual experience of seeing a series of schematic lines on the left which meant no and another one on the right which meant yes."

Deral, Card 2 (April 29, 1913) "Yes—no!" Time, 2.8 sec. Introspection: "My attention went to the color. After my initial focus

upon color, I was aware of a lapse in consciousness, of a sort of a break with no sense of the problem. At this time I had an affective toning of interest. Then I focused my attention on the left by a voluntary effort which appeared in a kinaesthetic shock. A vocal-motor image of 'acute' occurred, as well as kinaesthetic and visual imagery of balancing. I had a visual image of a figure as if hanging in space, the acute angle being supported by a wire and the two halves of the figure balancing perfectly. At the same time I had a kinaesthesis of balancing myself which was localized in the arms and hips. Then a vocal-motor verbal process 'belongs to first type.' Then my reaction 'yes.' Then the figure was withdrawn; my eyes looked downwards. Presently came a strong visual image of the first figure I had seen. I saw clearly the lower part of the figure and noted that the lower extremities of the two halves were separated widely. At this I was aware of a feeling of great strain in my eyes, as if they were diverging; my focus changed from the floor, with an almost painful feeling of my eyes being stretched apart. The kinaesthesis was actually stronger than the sensation of the change of focus. Immediately I said 'no.'"

Deral, Card 5 (April 29, 1913) "Yes." Time, 1 sec. Introspection: "My attention went to the base of the right-hand side. I was then aware of kinaesthesis of my eyes moving up the line adjoining the two halves; this was a muscular pull of my eyes following the line. Therewith I had a familiarity,—a memory of a movement like that in the past. The kinaesthesis was an immediate sort of a thing, an easy sensation, as a repetition of a movement made very recently. My reaction followed."

Deral, Card 12 (April 29, 1913) "Yes—not sure." Time, 1.2 sec. Introspection: "I based my decision upon the correct position of the foot. At the very outset my attention focused upon the lower right-hand part. This focusing was followed by my reaction 'yes.' After the figure was withdrawn, a visual image of it persisted. My attention in the image went to the union between the right-hand and left-hand sides; I do not think that the right-hand figure is inserted far enough into the left-hand figure. After my 'yes' a vocal-motor 'no angles' followed, attention passing to a visual image of an acute angle running into a mass of something. The acute angle was clear, but the mass was foggy and cloudy."

Observer D

Zalof, Card 3 (November 27, 1912) "It is not." Introspection: "I was aware of a visual image of one of the figures of the series, as soon as my eye fell upon the exposed figure. The image had a compact form, similar to that of the present figure; it was perhaps the seventh or eighth member of the Zalof series. I found my eyes moving about the margin of the exposed figure, and with the movements I counted the sides,—'one, two, three, four, five.' The sides were not sharply differentiated, but the arrangement was pentagonal. As soon as I became aware of this fact,—the counting, the pentagonality,—my negative response followed of itself."

Zalof, Card 5 (November 27, 1912) "It is." Introspection: "As soon as my attention fell upon the figure which was exposed, I had vague and fleeting visual images of several members of the original series. None of these images represented compact figures; all of them had elongated projections. I was aware of slight pleasantness, and my affirmative response followed. Immediately afterwards, I

was aware of doubt and unpleasantness, together with an awareness that the terminal projections did not agree with those of the members of the series. This last awareness consisted in fleeting visual images of the terminal processes of the original series, together with an awareness of the fact that the processes which I perceived did not correspond. I was aware of vocal-motor imagery of the word 'flipper,' as I attended to the visual images of the projections which I had just perceived; they were more like flippers than they were like the projections of the original Zalof figures."

Zalof, Card 7 (December 4, 1912) "It is." Time, 1.8 sec. Introspection: "I was aware at the outset of a rapidly alternating series of visual images of three of the original series. One of these images had very long arms, and I could see the pair of branches at the end of each arm. Another had arms of intermediate length. The third was much more compact than either of the others. All of this time I was conscious of the general shape of the present figure, and of its pair of terminal branches; my attention alternated rapidly between the percept and the imagery. My eyes passed in this way from the center to each terminal process, and I found that the paired arrangement was present in every case. I was aware of slight pleasantness, but of no intensive degree of strain or tension. Immediately after completing the fixation upon the ends of the branches my response came involuntarily."

Zalof, Card 9 (December 4, 1912) "I am somewhat in doubt." Time, 3.25 sec. Introspection: "First of all, I observed the general shape of the figure.—the fact that it had three arms. My attention went to the ends of the arms, and I found that smaller branches were present, and grew out of the two sides of each end; hence they exhibited a bi-partite arrangement. But I also observed that the arrangement was not that of a forking pair of branches at the end of these end-arms. I was aware of hesitation; I looked over the figure several times, always coming back to the ends of the branches, where I observed the bilateral arrangement of the terminal processes. I had a visual image of the first card, which clearly contained the common characteristics. Presently I said 'I am in doubt.'"

Zalof, Card 10 (December 4, 1912) "It is unmistakably a Zalof." Time, 2.45 sec. Introspection: "I was clearly aware of sweeping my eyes first to the upper left-hand corner, then to the right-hand corner, and then to the lower side; in each case I observed that the terminal pair of branches was present. This fact was in the focus of my consciousness. I was less clearly aware at the time of the triangular form of the figure, and also of the fact that the figure was very irregular. I was aware of no pleasantness or unpleasantness and of no tension. The dominant thing was the fact of the terminal branches; and I responded affirmatively."

Zalof, Card 2 (December 11, 1912) "It is." Time, 3.75 sec. Introspection: "I had not a high degree of certainty. First of all I observed the triangular form, then the bi-partite arrangement of the terminal arms. I was briefly aware of red coloring. Most of the time my attention was devoted to observation of the terminal processes. This observation was accompanied by a *Bewusstseinslage* of doubt and hesitation; and finally I found that I had responded affirmatively, without having deliberately made up my mind to do so. I believe that my lack of certainty was due to a doubt as to how many processes must be present at the ends of the branches

of the Zalof figures." (Describe your *Bewusstseinslage* of doubt.) "This consisted in a complex mental process which was dominately emotional in character and describable in some such terms as doubt, uncertainty, confusion and the like." (What was the emotion?) "Neither of the terms pleasant nor unpleasant does justice to the facts." (What other contents were present?) "As I remember it, the affective component was unpleasant; the other components of uncertainty and hesitation I can not now describe fully."

Zalof, Card 8 (December 11, 1912) "It is not." Time, 3 sec. Introspection: "My first glance at the figure revealed its general form. Then my attention concentrated upon the terminal arborizations, and I was distinctly aware of passing my regard from one corner of the figure to another, and of pausing at each corner to examine the number and arrangement of the terminal processes. I found the arrangement to be perfectly regular, and in fours; moreover, the minute terminal arborizations which I found in the original figures were now lacking. My consciousness of the regular arrangement and of the absence of minute terminal arborizations was followed by my negative response."

Deral, Card 1 (February 12, 1913) "Yes,—it all depends upon what is the color of the left-hand side." Time, 8.4 sec. Introspection: "I was aware of looking for certain characteristics,—the notch, the straight side, the color, and the further extension in a posterior direction of the colored side. My awareness of these characteristics was present in vocal-motor verbal terms. I found all of them excepting color; at first I was aware of a tendency toward a negative reaction; this was an emotional sort of a thing which I can not now describe. Then there appeared a series of vague and sketchy vocal-motor images of which the following is the purport: 'Yes, it is gray; but I think I can find a little blue in that gray, so I will say yes.' The verbal imagery was much more vague and sketchy and fragmentary than these words indicate. My attention was on the left-hand side of the figure."

Deral, Card 4 (February 19, 1913) "No." Time, 5 sec. Introspection: "I was aware of an *Aufgabe,*—a definite set or tendency,—to look for color on the left-hand side. I noted that this side was white but there was no tendency to react. My eyes swept to the other side which I saw was colored, a violet color; again there was no tendency to react. I began to seek for confirmation of other characters; the rectilinear 'snout' region at the base of the right-hand side attracted my attention. From the 'snout' characteristic my attention returned to the left-hand side; I was aware of a slight shock when it proved to be gray. My negative response followed."

Deral, Card 5 (February 19, 1913) "No." Time, 6.6 sec. Introspection: "I was aware of an *Aufgabe,*—a tendency to look for color on the left side. I found color in this region. Next I observed the two rectilinear sides; I was conscious of them only for a moment. My attention was next attracted to the right-hand side, where I became aware for the first time that the hairs were absent. I then tried to remember whether hairs were always present in the original figures; this consisted in a tending to say, repeatedly, 'did it have hairs?' I almost spoke these words aloud. Before the problem was settled my attention was attracted to the blotches on the right-hand side of the figure and I reacted immediately in negative fashion."

Deral, Card 8 (February 26, 1913) "It is not." Time, 3.2 sec. Introspection: "First of all, I noted the presence of hair around the border; and I tended to react affirmatively. As far as I can remember, this tendency consisted in an internal and organic welling-up; it also consisted in a tendency to reach out my right-hand, which was lying in my lap. My attention then went to the left-hand side, where I found the rectilinear edge. My attention paused. It then swept back to the right side which I found to be colored. The next thing I knew I had reacted negatively."

Deral, Card 11 (February 26, 1913) "Yes, I think so." Time, 7.4 sec. Introspection: "First of all I examined the color, then the rectilinear side on the colored half, and then the short rectilinear side at the base of the uncolored half. I next noted the dots on the uncolored half and examined these very carefully, noting that they were dots and not hairs. Finally I reacted positively with much confidence; this confidence consisted largely in pleasantness. As soon as I had reacted, I remembered visually that this figure had possessed no cilia; immediately I added 'I think so' to my response."

Deral, Card 12 (February 26, 1913) "Yes." Time, 5.6 sec. Introspection: "First I attended to the hair on the right side. Then I looked at the left-hand side. I found numerous short hairs in parallel rows. Then I attended to color, and then to the rectilinear 'snout' at the base of the right-hand side. All of this time I was aware of an increasing feeling of pleasantness; and finally I found that I had reacted with a good deal of confidence. Afterwards I remembered that I had failed to note whether the notch was present and also the long straight side in the left-hand periphery."

Deral, Card 13 (April 16, 1913) "Yes." Time, 6 sec. Introspection: "My attention went first to the left-hand side; I remember that it was blue and very much smaller than the right-hand side. I was next aware of a series of three visual images of figures which I have seen in the original series, together with vocal-motor verbal imagery of 'rectilinear side.' Then followed a period of hesitation, during which each of these visual images was in turn compared with my perceived figure,—*i. e.,* each image was in turn projected beside the objective figure. I found in each case that both image and percept possessed two rectilinear sides; in each case the coloring was on the appropriate half. Finally I found that I had reacted affirmatively."

Deral, Card 17 (April 16, 1913) "I am in doubt." Time, 4 sec. Introspection: "I first observed the left-hand side and found it colored; my regard then passed to the right, which I found to be covered with dots. I then noticed the rectilinear side of the anterior region of the left-hand part. My regard then swept to the right and I observed the lower rectilinear side. As yet I was aware of no tendency to react affirmatively. Then my eyes swept back and I found that the median line was rounded and not pointed as it usually is. I hesitated, and finally replied 'I am in doubt,' because I could not remember whether the notch must always have a point." (Describe your hesitation.) "I was not aware of visual images. My hesitation simply consisted in a tense uncertainty and suspense, which was unpleasant. It was not a bodily tenseness."

Deral, Card 18 (April 16, 1913) "Gee whiz! I am in doubt." Time, 8 sec. Introspection: "My attention was first attracted to the colored half, where it was held for a moderate interval. My regard then swept across the figure, and I noticed that the other half was

uncolored. I observed next the two rectilinear sides and then the notch. I became aware of doubt and tenseness; presently my regard and attention returned to the colored left-hand part. I was aware then of hesitation, which amounted almost to a mental paralysis; my regard could not be moved, and an inhibition of some sort occurred. I was not aware of any effort to compare the colors with those of the members of the Deral series, nor was I aware of any verbal imagery of 'uniform color' and the like. My awareness of the color was accompanied simply by this mental tension and unpleasantness. I then gave my reaction."

Kareg, Card 3 (March 5, 1913) "Yes, I think so." Time, 10 sec. Introspection: "First of all my attention was attracted to the left-hand part. I then saw the polylateral form, but I observed that one of the sides was concave. This concavity attracted my attention for a considerable time, and my observation of it was accompanied by slight tension and unpleasantness. My eyes then swept to the connecting section and then to the right-hand part. Presently they returned to the concave side. I noted the greater thickness at the center than at the periphery, and finally responded affirmatively, my uncertainty having to do with the concave side."

Kareg, Card 5 (April 24, 1913) Observer turns away. Time, 1 sec. Introspection: "My glance fell first upon the region of the left-hand side, which ordinarily adjoins the upper boundary of the connecting part. From this point I started to count; and I saw that the rest of the figure was lacking. Nevertheless, I later continued my counting; this may be explained perhaps by the fact that the sides were so well-differentiated. I was clearly aware of the presence of six sides."

Observer E

Zalof, Card 2 (November 18, 1912) "Yes." Introspection: "My attention went in turn to the triangular body, the limbs, and the red nucleus. In each case, my impression was accompanied by a vocal-motor verbal image, in the first case of 'triangular body,' then 'three limbs breaking up at end,' and finally 'red nucleus.' At the time of my observation of the central nucleus, I had a visual image of the central parts, the circle surrounded by the pear-seed-shaped bodies. When that came up, I unhesitatingly said 'yes.' Almost from the beginning I was aware of a tendency to react 'yes,' in vocal-motor terms."

Zalof, Card 3 (November 18, 1912) "No." Introspection: "As soon as my regard fell upon the exposed figure, I was aware of a vocal-motor verbal image of 'three limbs branching at end' and also of a visual image of a member of the original series, which had three long limbs. I then had a vocal-motor tendency to react 'no.' Then I became aware of the nucleus; I observed this long enough to see that it contained five parts. My negative reaction followed."

Zalof, Card 4 (November 18, 1912) "No." Time, .75 sec. Introspection: "At the very outset I was aware of a vocal-motor image of 'three limbs;' then my perception of the figure became clearer; almost immediately I had a vocal-motor image of 'four limbs' and also an image of 'no red nucleus.' My 'no' followed upon my perception of the four limbs."

Zalof, Card 7 (November 25, 1912) "Yes." Introspection: "At the very first moment, I decided that the figure was a Zalof. This

decision followed upon my perception of the general form and the red nucleus, and it consisted in a comparison of my percept with a visual image. A vocal-motor 'yes' appeared, but was inhibited by a closer observation of the nucleus. The visual image became clearer in consciousness than my actual percept, and I was aware of turning my attention from the visual image to the percept, and of examining the nucleus. This examination was difficult on account of the small size and indefinite drawing; but after I had observed the center, I decided that it was a Zalof and responded with 'yes.' Just before the stimulus disappeared I noted that each arm split into two terminal divisions. I was aware of a vocal-motor image of 'three arms.'" (Describe your first visual image.) "It was similar in form to the stimulus. It was a vague sort of a thing, and had rather long tentacles. My first vocal-motor 'yes' occurred before this visual image became very clear in consciousness." (Describe your last decision that it was a Zalof.) "This was nothing more than comparison with my visual image."

Zalof, Card 8 (November 25, 1912) "No." Introspection: "I observed the shape of the figure,—the triangular body, the three tentacles,—and was aware of a vague visual image of a Zalof. With my awareness of the form, I had a vocal-motor image of 'yes.' My eyes then focussed upon the nucleus, and the visual image cleared up with respect to its nucleus. A vocal-motor image of 'not similar' occurred. The rest of the time before my reaction was taken up in an examination of the center of the stimulus, in an effort to find whether anything occurred similar to the center of the image. The lines in the center of the stimulus were confused, and I could not be sure as to the nature of the central parts. My examination was accompanied by strain, which was localized in my eyes and forehead; I finally found that the perception did not correspond to my visual image, and I reacted negatively."

Zalof, Card 10 (November 25, 1912) "Yes." Introspection: "My awareness of the shape of the stimulus was accompanied by a visual image of a very long-armed Zalof. The stimulus had no triangular body; it was all arms, and I was aware of a tendency to say 'no.' My attention shifted to the nucleus, which I had difficulty in observing; I was aware of strain in my eyes and brow, and of a forward movement of my body. A visual image of a nucleus appeared; the image and the percept corresponded, my attention being primarily upon the nucleus. I was aware of unpleasantness, and finally of vocal-motor imagery of 'very ugly.'"

Zalof, Card 6 (January 13, 1913) "No." Time, 1.6 sec. Introspection: "My attention went first to the general shape, and I was aware of a vocal-motor image of 'no.' Immediately this was inhibited, the inhibition consisting in the appearance of a vocal-motor image 'see nucleus.' Then I observed the nucleus, and was aware of vocal-motor images of 'blue' and 'long-shaped.' My negative reaction followed."

Zalof, Card 14 (January 13, 1913) "Yes." Introspection: "First of all my attention went to the nucleus. I found that the three apple-seed-shaped bodies were not separated and I hesitated upon this feature. Then came a vocal-motor image 'look at rest,' and I was aware of an attitude of desiring to exclude the figure on the basis of other characteristics, but of being reluctant to exclude it upon the basis of the nucleus alone. My attention went to the other features, and presently I was aware of a verbal image, vocal-motor, of 'rest right.'

My fixation returned to the nucleus, and the verbal imagery appeared 'can not throw it out on that.' My affirmative reaction followed." (Describe your attitude.) "I was aware of uncertainty about the nucleus; I could not clearly distinguish its parts. My attention then went to other parts of the figure."

Zalof, Card 17 (January 25, 1913) "Yes." Time, 2.6 sec. Introspection: "My attention went first to the pseudopodia. I was aware of a vocal-motor image 'arms right; are branched.' My attention then shifted to the nucleus. Then came a vocal-motor image of 'right shape; not red,' followed by 'doesn't need to be.' Then came my affirmative reaction."

Zalof, Card 13 (January 27, 1913) "No." Time, 2 sec. Introspection: "At the outset my attention happened to fall upon the nucleus, and I was aware of a vocal-motor image of 'no nucleus.' My reaction 'no' followed before I noticed the extremities at all."

Deral, Card 3 (December 9, 1912) "No." Time, 1.75 sec. Introspection: "My attention went to the central part of the uncolored side. The vocal-motor verbal image 'no point' occurred; then came the vocal-motor image 'one orange.' A visual image of a correctly pointed central line occurred. I observed that the central line between the two halves of the percept bulged toward the left. Toward the close of my observation I was aware of strain in my forehead, and of mild unpleasantness. My reaction followed. Vocal-motor imagery of 'ugly' occurred."

Deral, Card 5 (December 16, 1912) "Yes." Time, 5.6 sec. "I was aware of a series of rapid changes of fixation, or at least of attention, and as my regard fell upon a part of the figure I named it in vocal-motor images. The vocal-motor images were as follows: 'point,' blue,' 'square edged,' 'pseudopodia.' Then came a vocal-motor 'yes' which was not inhibited." (Were you aware of any 'yes' attitude before your inhibited 'yes?') "Yes. After my verbal image of 'blue' I was aware of a vocal-motor image of 'yes' which persisted."

Deral, Card 7 (December 16, 1912) "Yes." Time, 2.75 sec. Introspection: "I had great difficulty in observing the figure, because it was so small. I was aware of straining forward. My attention shifted without shifts of fixation; as it fell upon certain parts of the figure, vocal-motor images appeared. They were as follows: 'right pointed;' 'left notch, colored;' 'straight edge;' 'pseudopodia off.' Somewhere during this process the vocal-motor image 'yes' occurred which was inhibited by my attraction of attention to other features. I was also aware of vocal-motor imagery 'very small,' and then of 'size does not matter.'"

Deral, Card 9 (January 11, 1913) "Yes." Time, 12.25 sec. Introspection: "At the very first, I was aware of a vocal-motor image of 'yes,' which was inhibited by my effort to find a well-defined straight line in the lower left periphery of the figure. This line was straight, but it did not break off sharply, as it should. The rest of my observation consisted in a series of vocal-motor images, representing an argument as to whether I could decide that the lower line was straight enough to call the figure a Deral. I also verified my other elements, in shifts of attention and corresponding vocal-motor verbal images. The following were the images: 'left colored, right not;' 'pseudopodia;' 'point;' 'notch.' My attention then returned to the lower left." (Describe your vocal-motor verbal argu-

ment.) "I can remember the following verbal images: 'isn't square;' 'is square;' 'doesn't end sharply; is ending sharply necessary? Always was in others; was not in my definition; so we will call it Deral.'"

Deral, Card 2 (January 18, 1913) "Yes." Time, 3.2 sec. Introspection: "My attention went first to the left-hand figure, and the vocal-motor image of 'some brilliant red' appeared. I looked at that red for a while, simply as red. Then my attention shifted, and as it fell upon various parts of the figure, vocal-motor images representing those parts occurred, as follows: 'lower line;' 'notch;' 'striations.' Then my affirmative reaction followed."

Deral, Card 4a (January 18, 1913) "Yes." 11.2 sec. Introspection: "First of all my attention went to the form of the figure and a vocal-motor image of 'turned aside' appeared. I then verified all of my essential features in terms of shifts of attention and corresponding vocal-motor images as follows: 'color;' 'straight edge;' 'notch;' 'striations.' Then for a time I considered as to whether the statements of my definition, that the right-hand side was uncolored and the left-hand side was colored, could be changed so as to admit of an upper side being colored and a lower side being uncolored. This consideration was vocal-motor verbal, essentially, but I can not now remember all of the imagery. Throughout it my attitude was to include rather than to exclude. The verbal images which I can remember were 'definition right-left, this upper-lower.' 'Possible to consider merely laid on side.' 'Lower is to right, so can call it Deral.'" (How did that attitude appear?) "It appeared in an effort to find reasons for including the figure, rather than for excluding it."

Deral, Card 17 (January 27, 1913) "Yes." Time, 6.8 sec. Introspection: "My attention passed over the figure, and the points upon which I fixated were characterized in vocal-motor images as follows: 'straight edge;' 'colored;' 'curve;' 'notch.' Then my attention went to the median line, and the vocal-motor imagery of 'but has a notch, point isn't sharp but is there.' I was aware of strain and tension in my brows and eyes. Finally my affirmative response followed."

Deral, Card 18 (January 27, 1913) "Yes." (Frowns strongly.) Time, 4 sec. Introspection: "My attention went immediately to the color of the left-hand body. A vocal-motor image of 'good Lord!' appeared as I observed this. My attention then went to other features, and as my fixation fell upon them verbal images occurred, as follows: 'straight edge;' 'curved;' 'notch and point;' 'detached bodies;' 'pseudopodia;' 'shape right.' My attention then returned to the color, and I was aware of a vocal-motor consideration as to whether I could include the figure in spite of its color. I can remember the following imagery: 'definition says, left colored, color may vary.' 'According to the definition it is a Deral.' 'Should like to change definition to read 'left colored but color same for a given individual.' I was aware of extreme unpleasantness. Finally my affirmative response followed." (How completely was your vocal-motor consideration of the color present?) "It was rather complete. I can not give it exactly."

Tefoq, Card 1 (February 3, 1913) "Yes." Time, 12 sec. (Observer frowns markedly after 9 sec.) Introspection: "I was aware of shifting of attention to various parts of the figure, and of verbal images corresponding to the parts upon which my attention fell. The following were the verbal images; 'central body right;' 'green;' 'crow's

foot;' 'violet bodies.' With the last I was aware of hesitation; verbal imagery of 'don't know whether' appeared, and I was aware of wondering whether the presence of these violet bodies would make it necessary to exclude the figure. My attention then went to the periphery, and the following verbal images occurred (vocal-motor): 'circular;' 'cut, but small.' Then my attention went back to the violet bodies, and I was aware of more hesitation, of tension in my brows and of a straining toward the stimulus. Vocal-motor imagery of 'outer may be colored.' My affirmative reaction followed. Throughout my observation of this card, I was aware of an attitude of including the figure, if possible. My problem seemed to be, 'let's see if we can call this a Tefoq.'"

Tefoq, Card 5 (February 3, 1913) "Yes." Time, 8.2 sec. Introspection: "My attention shifted to various parts of the figure, and the points upon which it fell were characterized in verbal images, as follows: 'picture frame;' 'central body right.' My attention then went to the periphery. 'Too circular;' 'cut.' My attention then shifted to the triangular patch, and a vocal-motor image of 'small body' appeared. I hesitated upon the central part in the small body and finally appeared a verbal image of 'that's right,' followed by a return of my attention to the periphery and the background. The following vocal-motor images occurred: 'uncolored;' 'color may vary;' 'no pseudopodia,—not always pseudopodia;' 'too circular.' Finally, 'nothing in definition against this.' Then my affirmative response followed. I did not look for the crow's-foot."

Tefoq, Card 3 (February 8, 1913) "No." Time, 2.8 sec. Introspection: "My attention went to the outer edge, and never left it. The following vocal-motor images occurred: 'not circular;' 'general form circular;' 'pseudopodia; too confounded irregular.' Immediately my negative reaction followed. The experience was unpleasant in a mild degree."

Tefoq, Card 7 (February 19, 1913) "No." Time, 2.2 sec. Introspection: "My attention fell first upon the central body. The vocal-motor imagery of 'edges curved, should be sharp' appeared. My negative response followed, and I did not perceive the rest of the figure at all."

Tefoq, Card 10 (February 19, 1913) "Yes." Time, 8 sec. Introspection: "My attention fell upon the central projection and vocal-motor imagery of 'Gosh that's ugly!' occurred. I was aware of contraction of my brows and of unpleasantness. Then came vocal-motor imagery of 'let's do it systematically.' My attention went then in turn to all of my essential features, and as my fixation fell upon each point vocal-motor images appeared, as follows: 'sides green;' 'right shape;' 'top uncolored;' 'crow's-foot;' 'small triangle;' 'proper place.' My fixation then went to the circular body, and verbal imagery occurred as follows: 'shape right;' 'notch.' Then came verbal imagery of 'perspective awful;' 'no reason why it could not be.' Then came verbal imagery of 'got to say yes.' My affirmative response followed. I was aware of unpleasantness and frowning. My attitude throughout was that I would like to reject that figure but could not find any valid reason for doing so."

Tefoq, Card 12 (February 19, 1913) "Yes." Time, 6 sec. Introspection: "Noted successively different parts of the figure which were represented in vocal-motor imagery at the time, as follows: 'crow's foot;' 'shape;' 'green;' 'circular;' 'notch.' My affirmative response

followed. Immediately afterwards I was aware of a visual image of the figure, and I am quite sure that the small blue triangle was missing. Before the exposure terminated I did not look for it or notice it." (What was the immediate antecedent of the appearing of your visual image? "I do not know. It just came in while I was thinking about the shape of the figure, before introspecting."

Kareg, Card 4 (March 5, 1913) "No." Time, 4.6 sec. Introspection: "This was an exceedingly unpleasant experience. My fixation went first to the left-hand body; it remained there, and the rest of the figure was not perceived. I was then aware of a highly increased mental activity, in which I considered in vocal-motor verbal fashion, whether the two-sided pyramid was possible for a Kareg figure. Although there was nothing in my definition against it, I insisted that the pyramidal Kareg figures had had either five or three sides and never two sides. I can remember the following vocal-motor image: 'can not throw it out, but none like it.' On the basis of this awareness that none of the figures were like the present one, I rejected it. Throughout my observation, I had an attitude of wanting to throw out the figure, if possible. This was largely emotional."

B. THE MAIN PROCESS OF CLASSIFYING. 1. *Nature of the Process.* In every case, the observers' classifyings were constituted by a peculiar and more or less persistent course or direction of consciousness, which consisted in the fact that attention passed successively to those regions of the classification-figure whose counterparts in the generalization-series had contained essential features. If the feature were definitely present (objectively) in the classification-figure, it stood out briefly and easily in the observer's attention; and the course of consciousness continued without interruption. In many instances, the conspicuous and first-established general features (when definitely present) were noted in exceedingly brief fashion, usually without a high degree of attention; the observers were then vaguely aware of the general shape of the figure. The finer and less gross features, when obviously present, usually stood out in somewhat greater clearness; this would follow, of course, from the mere fact that the observer was obliged to spend slightly more time in his examination in order to make sure of their presence.

This course of attention persisted either until the regions of all the essential features had been examined, or until an essential feature failed to stand out rapidly and clearly as the regard passed to its region, or until a striking non-essential feature attracted the attention. In either of the two latter cases, the region in question became unusually focal. If an essential feature were obviously absent, the course of the attention was usually terminated abruptly; sometimes, however, it resumed its course after the interruption.[6] If the essential feature were neither definitely present nor definitely

[6] *Cf.* Cautious classifications, p. 93.

absent, the attention was arrested for a longer or shorter time upon the region; and strains, tensions, and unpleasantness often made their appearance. After a time, the normal course of consciousness was resumed, usually in a less rapid and facile manner—*i. e.*, the observers proceeded cautiously.

In the great majority of cases, the observers' attention went first to those regions of the grosser general features which had first been established as essential. Yet the observers never reported that they searched for these regions; instead, the features themselves, if present, flashed out in consciousness at the first glance, whereupon the observation continued. With the Zalof cards, for example, the observers usually attended first to such conspicuous features as the mere triangularity, or the general outline, or the form of the central parts; and later they explored the tentacle terminations, or other finer details. When a conspicuous essential feature was present in altered form, the observers found that their regard was fastened at the outset upon its region. If an inconspicuous general feature were altered, the observers usually did not become aware of the fact until attention, in its ordinary course, reached the altered region.

Thus the main process of classifying—the observers' manner of perceiving the classification-figures—was characterized for consciousness by a passing of attention in more or less rapid succession to the regions of the essential features, *i. e.*, by a successive definitizing and focalizing in consciousness of these regions. This may be regarded as an attention-activity of questioning,—'Does it have the general features?'; and the ready standing-out of the feature itself, or its failure to stand out, when attention passed to its region, constituted respectively the affirmative and negative answers to the question. When an essential feature neither stood out in ready fashion nor failed definitely to appear, the observers gave their answers in a hesitant fashion.

2. *Component and Concomitant Processes of the Main Process of Classifying. a. Perception of the figure:* In many cases the process of classifying operated solely upon the visual structural basis furnished by the perception of the exposed figures.[7] *b. Additional contents:* Very frequently,

[7] *Cf.* especially *A; affirmative,* Zalof, Cards 7 and 10, p. 62, p. 63; Kareg, Card 1, p. 65; *negative,* Zalof, Card 3, p. 62; Tefoq, Card 4, p. 64; Card 11, p. 64; Kareg, Card 8, p. 65; *hesitant,* Zalof, Card 11, p. 63. *B: affirmative,* Zalof, Card 17, pp. 68f, first part; *negative,* Zalof, Card 9, p. 66; Card 12, p. 67; *hesitant,* Tefoq, Card 11, p. 70. *C: affirmative,* Cards 8 and 5, p. 73; *negative,* Zalof, Cards 9 and 13, p. 73. *D: affirmative,* Zalof, Card 10, p. 76; Deral, Card 12, p. 78; *negative,* Zalof, Card 8, p. 77; Deral, Card 8, p. 78; *hesitant,* Zalof, Card 2, p. 76; Deral, Card 18, p. 78.

however, the classifying was not so simple as this mere play of attention upon the classification-figure itself. In such cases, the observers reported the presence of other components besides their visual perceptions of the exposed figure. These additional components were of two sorts: *a*. Imaginal or sensory contents which reinforced the observers' findings during their examinations of the stimulus-figures themselves; and *β*. Imagery of the original series.

α. The contents which reinforced the observers' findings were usually verbal, but often non-verbal kinaesthetic. When verbal, they were usually identical with verbal images which had previously occurred during the first and later examinations of the generalization-figures. These images emerged as the observer's glance fell upon the corresponding region of the figure; and they appeared spontaneously and rapidly, usually with no specific recognition or conscious reference to the original series. Apparently they referred only to the present figure, whose examination they reinforced. The verbal imagery varied, according to whether the essential feature was present or absent in the region observed. When this feature was definitely present, the verbal imagery usually consisted merely in the verbal characterization itself.[8] Sometimes, however, the imagery included words which expressed the correctness of the feature, as it stood out (*E,* Zalof, Card 17, p. 81). Again, the verbal reinforcing imagery occurred in a slightly changed form, as if it were an answer to a question: it stated that a figure possessed a specified essential feature (*B,* Tefoq, Card 4, 'steps washed in green,' 'triangle blue,' pp. 69f). When, on the other hand, an essential feature was dissimilar or definitely absent, the verbal reinforcing imagery included words which expressed the absence or dissimilarity of the feature.[9] Or the verbal images were not identical with previously-employed designations, but instead they consisted in words which specified the nature of an altered region in the stimulus itself.[10]

The non-verbal kinaesthetic reinforcing contents consisted sometimes in imagery of the eye-movements of examining the figure,[11] and sometimes in internal imitations of the figure (*cf.* p. 103) which were often marked by discomfort and unpleasantness.[12]

β. The imagery of the original series was usually concrete visual, but sometimes verbal. When visual, this imagery made its appearance sooner or later after the exposure of the stimulus; the observer compared it with the stimulus figure, attending alternately to corresponding regions of percept and image. Thus the main classifying-

[8] *B,* Tefoq, Card 3, p. 69; Card 4, pp. 69f. *E,* Zalof, Card 2, p. 79; Deral, Card 5, p. 81; Tefoq, Card 12, pp. 83f.

[9] *B,* Deral, Card 1, 'not colored,' pp. 70f. *E,* Zalof, Card 4, 'no red nucleus,' p. 79; Card 13, 'no nucleus,' p. 81; Deral, Card 3, 'no point,' p. 81.

[10] *B,* Zalof, Card 6, 'has rounded corners,' p. 66; Deral, Card 1, 'black,' p. 71. *E,* Zalof, Card 4, 'four limbs,' p. 79; Card 6, 'blue,' 'long-shaped,' p. 80; Tefoq, Card 7, p. 83. *C,* Zalof, Card 6, p. 72.

[11] *B,* Zalof, Card 1, pp. 65f; Card 4, p. 66. *C,* Zalof, Card 11, p. 73; Deral, Cards 1 and 5, pp. 74, 75.

[12] *C,* Deral, Card 8, p. 74; Zalof, Card 2, p. 72; Card 4, p. 72; Kareg, Card 2, p. 73.

process ran its course in terms both of percept and image. The imagery was often relatively complete, sometimes presenting those members of the original series which resembled the stimulus, and sometimes presenting the extremes, in size and form, of the original (generalization) series. Again, the concrete visual imagery was fragmentary, presenting that part of the original figures which was dissimilar or absent in the stimulus.[13]

The occurrence of concrete imagery of the original groups during the observers' examinations of the classification cards may be correlated with two conditions: *a.* The number of times the observers had examined the generalization-groups, or the number of their classifications upon past occasions; and *b.* the nature of the classification-judgment,—whether facile (affirmative or negative) or hesitant. No observer ever reported the invariable presence of imagery of the original groups with any one type of judgment; and the relative number of the judgments of any one of the three varieties which actually involved such imagery varied widely with the different observers. For a more complete treatment of this subject, *cf. Individual Differences,* pp. 106 ff.

The verbal imagery of the original series sometimes accompanied the concrete visual. It invariably consisted in words characterizing an essential feature which was lacking in the stimulus.[14]

3. *The Initiation of the Main Process of Classifying.* The classifying-process was usually anteceded only by the hearing of the experimenter's question: 'Is this a Zalof (or Deral, etc.)?' and by the immediate appearance to consciousness of the figure for classification. No observer ever reported a definite *Aufgabe*-consciousness of accepting the task, or of setting up a goal-idea, or of self-instruction to adopt a certain procedure, or of explicit questioning—'will it have the Zalof (or Deral, etc.) essentials?' Instead, the questioning response of attention which was characteristic of the classifying-process followed immediately upon the perception that the stimulus was exposed.

Under certain conditions, however,—when the attention was arrested by a striking variable or by a markedly dissimilar essential, or when a premature tendency to respond had occurred,—the observers occasionally reported the presence of concrete or verbal imagery which was followed by a turning of attention to a new region, *i. e.,* which initiated the reinstatement of the main classifying-process. This imagery usually presented a part of the figure which had not yet been investigated. When this was the case, it was sometimes visual (*B,*

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[13] *Cf. A: affirmative,* Deral, Card 4, p. 64; *hesitant,* Deral, Card 2, p. 64; Tefoq, Card 13, p. 65. *B: negative,* Zalof, Cards 6 and 17, pp. 66, 68; *hesitant,* Zalof, Card 10, p. 67; Card 2, p. 68; Tefoq, Card 1, with attention to background, p. 69; Card 10, p. 70. *C: affirmative,* Zalof, Card 7, p. 72; *negative,* Kareg, Card 8, p. 73. *D: affirmative,* Zalof, Cards 5 and 7, pp. 75, 76; *hesitant,* Zalof, Card 9, p. 76; Deral, Card 13, p. 78. *E: affirmative,* Zalof, Card 2, p. 79; *negative,* Deral, Card 3, p. 81; *hesitant,* Zalof, Card 8, p. 80; Card 10, p. 80.

[14] *B,* Zalof, Card 6, 'bifurcation,' p. 66; Tefoq, Card 5, 'hi-' (hieroglyphic), p. 69. *E,* Zalof, Card 4, 'three limbs,' p. 79.

Deral, Card 2, noting of right periphery, p. 71); more often, however, it was verbal,—either consisting of words which designated the essential feature subsequently investigated (*B,* Tefoq, Card 6, 'green wash,' p. 70), or taking the form of definite self-instruction to note the feature (*E,* Zalof, Card 6, 'see nucleus,' p. 80; Card 14, 'look at rest,' p. 80). When the imagery did not present a feature which was subsequently investigated it consisted in verbal self-instruction (*E,* Tefoq, Card 10, 'let's do it systematically,' p. 83) or in other kinaesthesis (*C,* Deral, Card 2, shock, p. 75). In the great majority of cases, however, no imaginal antecedents whatsoever marked the reinstallation of the classifying-process.[15] Here the series of examinings of the regions of essential features merely reasserted itself after a more or less prolonged period of interruption. In the light of these instances, it seems highly probable that when the re-initiation of the classifying-process was immediately preceded by concrete visual imagery this latter is to be interpreted as constituting the initial term of the process as reinstated, rather than as an *Aufgabe*-consciousness, an awareness of intending to continue the investigation. That is, the process re-commenced in concrete imaginal terms, instead of in perceptual terms.

It occasionally happened that an observer verbalized a classification-judgment before his survey of the figure was complete. Under these conditions he usually became aware, immediately afterwards, of the inadequacy of his observation, in terms of vague imagery of that part of the figure which had not been investigated; and a renewal of the process of classifying was initiated. When such images occurred before the figure was completely removed from view, they were followed by a rapid passing of the regard to that part of the figure which had previously escaped notice. If, however, the figure could no longer be seen, the imagery was followed by an effort to make a careful examination of the neglected region, in a visual image of the stimulus,—an effort which was not always successful.[15] At other times the imagery in question was accompanied by strain and affective content; and the whole functioned as a regret that the response had been given so soon (*C,* Zalof, Card 7, p. 73). Again, the imagery functioned—with or without the additional kinaesthetic and affective contents—as a desire to see the figure again, or merely as an awareness that certain parts of the figure had not been seen (*E,* Tefoq, Card 5, p. 83. *D,* Deral Card 12, p. 78). Even in such cases, however, it usually happened that the main process of classifying was rehabilitated spontaneously—operating in verbal or visual imaginal terms—with no antecedent contents whatever.[16] Here, too, then, it seems most probable that the antecedent imagery, where it occurred, is to be regarded as an initial term of the reinstated process of classifying, rather than as an *Aufgabe* or intention. Thus the only instances in which unequivocal contents of intending to investigate certain parts of the figure were present were the few cases, cited above, where observers reported verbal images of self-instruction, or contents which did not present parts later investigated. The continuous sequence of experiences which constituted the process of classifying followed immediately upon the awareness that the exposure had been

[15] *C,* Zalof, Card 7, p. 72; Deral, Card 12, p. 75. *A,* Zalof, Card 10, p. 63.

[16] *A,* Deral, Card 4, p. 64; *B,* Zalof, Card 1, p. 65; Card 10, p. 67; Card 12, p. 67; Card 17, p. 68; Tefoq, Card 4, p. 69. *C,* Deral, Card 2, p. 74. *D,* Deral, Card 11, p. 78.

made, after the hearing of the verbal instructions; it was never instigated by the occurrence of self-instruction of any sort, and only upon rare occasions did such instruction mediate its reinstatement after an interruption.

C. THE FINER COMPONENTS OF THE PROCESS OF CLASSIFYING: THE BEHAVIOR IN CONSCIOUSNESS OF THE FEATURES OF THE CLASSIFICATION-FIGURES. Thus far we have examined the process of classifying in its larger aspects,—its general nature, its structural aspects, its manner of initiation. We shall now consider its finer component processes. These latter consist in the behavior in consciousness of the specific regions over which or to which attention passed in its main course over the figure.

1. *Behavior in Consciousness of Obviously Present Essential Features: Facile Affirmation.* *a. Normal:* As has been pointed out, when asked to state whether a figure was a Zalof (or Deral, etc.), the observers adopted a mode of observation which in itself constituted a behavior of questioning 'does it possess the essential features?'; *i. e.,* the attention passed in more or less rapid succession to those regions of the figure which had been found to contain essentials. If a feature which the observer had found to be common was definitely present in the exposed figure, it flashed out into clear consciousness as the attention passed to and over its region; and this facile and often very brief standing-out of the feature constituted in itself the affirmative answer to the question which was constituted by the peculiar course of attention. Such a standing-out of an essential feature was sometimes accompanied by additional imagery (*cf.* pp. 86 ff.), and it was frequently followed by an affirmation of some sort (*cf.* p. 91). If the observation had not as yet been completed, the attention shifted easily and readily to other parts of the figure, without any delay upon the region in question; and if the observer did not note the absence of any essential feature, an affirmative response followed. The latter was sometimes verbalized before the course of attention reached its culmination.[17] (*Cf.* Premature Facile Affirmations, pp. 90 ff.).

[17] *Cf.* the following classifications: *With no structural content other than the percept itself, A,* Zalof, Cards 5, 7, and 10, pp. 62, 63; Kareg, Cards 1 and 5, p. 65; Deral, Card 5, p. 64. *B,* Zalof, Card 17, p. 68. *C,* Zalof, Cards 8 and 5, p. 73. *D,* Zalof, Card 10, p. 76; Deral, Card 12, p. 78. *With imagery of the original series* (*concrete visual*), *A,* Deral, Card 4, p. 64. *C,* Zalof, Card 7, p. 72. *D,* Zalof, Card 7, p. 76. *E,* Zalof, Card 2, p. 79.

When verbal imagery which reinforced or supplemented the observers' findings was present (*cf.* pp. 86 ff.) it took the form of

b. Premature Facile Affirmations: The reader will have noticed that observers frequently responded prematurely, before the attention had passed to the regions of all the essentials. In such cases, the affirmation which was aroused by the standing-out of the first essential or the first few essentials was followed by an affirmative spoken response before the course of the process of classifying was completed, or in other words, the affirmative reaction-tendency here overcame the main classifying-process. Affirmative classifyings of this sort are doubtless to be regarded as exaggerated forms of the classifyings in which a tendency to say 'yes' occurred before the close of the observation but was not actually followed by a spoken 'yes.' When the 'yes' was spoken in this premature fashion, however, the main process of classifying immediately reasserted itself, and it operated upon a basis of imagery of the stimulus, if the exposure had already terminated. That is, the attention passed to those parts of the image whose fellows in the percept had not been investigated. The observers were sometimes able to complete their classifyings in a glance at the disappearing stimulus, or else their imagery was definite with respect to the features which had not yet been investigated; when this was the case, they then proceeded to correct or to modify their premature response, or to indicate in some way that they were aware of the nature of the uninvestigated feature.[18] Sometimes, however, the observer's imagery was indefinite with respect to the uninvestigated parts; and he merely indicated in some way his awareness that his observation of the stimulus had been incomplete.[19]

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naming the feature which stood out at the moment; and the naming was sometimes accompanied by words which expressed the correctness of the feature: *E,* Zalof, Card 17, p. 81. *Cf.* also the following affirmative classifications, in which the behavior in consciousness of the essential features was complicated by the presence of supplementary imagery: *Verbal, B,* Tefoq, Cards 3 and 4, pp. 69 ff. *E,* Zalof, Card 2, p. 79; Tefoq, Card 12, p. 83. *Kinaesthetic of eye-movement, B,* Zalof, Card 1, p. 66. *C,* Zalof, Card 11, p. 73; Deral, Cards 1 and 5, pp. 74, top; 75. *Kinaesthetic of internal imitation, C,* Deral, Card 8, p. 74; Zalof, Card 2, p. 72.

[18] *A,* Zalof, Card 5, p. 62; Deral, Card 4, p. 64; Kareg, Card 5, p. 65. *B,* Zalof, Card 1, p. 66; Card 17, pp. 68f; Tefoq, Card 4, p. 70. *C,* Deral, Card 2, p. 75; Card 8, p. 74; Card 12, p. 75. *D,* Zalof, Card 5, pp. 75f; Deral, Card 11, p. 78. *E,* Tefoq, Card 12, p. 84.

[19] *A,* Zalof, Card 10, p. 63. *C,* Zalof, Card 7, p. 72. Sometimes the observer mentioned that his observation had not been complete, without describing his awareness of this fact; it seems highly probable, however, that this awareness was not essentially different from that present and described on other occasions. *Cf. D,* Deral, Card 12, p. 78. *E,* Tefoq, Card 5, p. 83.

c. Cautious Affirmations: In many cases the observers reported that they had proceeded more cautiously, that the course of their attention was slower, and that the features stood out more clearly as they were successively noted. The more detailed and obscure features were here noted in a still more painstaking fashion. In the Zalof figures, for example, the observer examined the arrangement of the tentacle-ends of each of the three projections, instead of being satisfied with a single glance at one of them. As regards the conscious nature of the affirmative response itself, and the extent to which and the manner in which the behavior of the feature in consciousness was accompanied by other contents, the cautious affirmative reactions were similar to the other affirmative reactions. Indeed, the two sorts of classifications were often very difficuit to distinguish; it was almost impossible at times to draw a line between the slower 'facile affirmations' and the more rapid 'cautious affirmations.' We have undertaken to make the distinction chiefly because it was found to constitute a useful basis for pointing out certain differences in observation-type among our observers (*cf.* pp. 105f, 108f, ff.).[20]

2. *The Affirmative Response:* The standing-out of the essential characteristics, constituting as it did the affirmative answer to the question present as the peculiar behavior of attention during the observation of the classification-figures, was in the great majority of instances followed only by a turning-away from the stimulus and a verbalizing of an affirmation; rarely, the latter was preceded by imagery of 'yes.'[21] The affirmative response was sometimes imaged in verbal terms after only one or more essential features had stood out, before the course of the observation was complete; it was inhibited by the continuation of the course of attention.[23] On rare occa-

[20] The following introspections illustrate the cautious type of facile affirmation; many of them have been referred to in previous paragraphs, but in later classifications they have been regarded as cautious: *A,* Deral, Card 5, p. 64. *B,* Deral, Card 1, as far as mention of negative tendency, pp. 70f. *D,* Zalof, Card 7, p. 76; Card 10, p. 76; Deral, Card 11, p. 78; Card 12, p. 78. *E,* Zalof, Card 2, p. 79; Card 7, pp. 79f; Card 17, p. 81; Deral, Card 5, p. 81; Card 7, p. 81; Tefoq, Card 5, p. 83.

[21] *A,* Zalof, Card 5, p. 62; Card 7, pp. 62f; Card 10, p. 63; Deral, Card 5, p. 64; Kareg, Card 1, p. 65; Card 5, p. 65. *B,* Zalof, Card 1, pp. 65f; Card 17, pp. 68f; Tefoq, Card 3, p. 69; Card 4, p. 69; Card 7, 'yes-tendency,' p. 70. *D,* Zalof, Card 10, p. 76. *C,* Zalof, Card 2, p. 72; Card 11, p. 73; Card 5, p. 73; Card 13, 'yes-tendency,' p. 73; Kareg. Card 2, 'yes-tendency,' p. 73; Deral, Card 8, p. 74; Card 5, p. 75; Card 12, p. 75. *E,* Zalof, Card 17, p. 81; Tefoq, Card 12, pp. 83f.

[23] *E,* Zalof, Card 2, p. 79; Card 7, p. 80; Card 8, p. 80; Deral, Card 5, p. 81; Card 7, p. 81; Card 9, p. 81. *Cf.* also 'yes-tendencies' cited in footnote 21.

sions observers reported that, as the process of classifying proceeded and as feature after feature stood out in the course of the attention, a gradual relaxing of the close concentration occurred, and the spoken 'yes' followed at the termination of the investigation (*B,* Zalof, Card 9, the 'yes-consciousness,' p. 66). Observers occasionally reported that the successive standing-out of the essential features was marked by growing pleasantness, or that pleasantness preceded the response or yes-imagery.[24] And kinaesthetic and organic contents, with or without pleasantness, sometimes entered into the affirmation-experience. At times these contents consisted in a kinaesthesis of tapping or waving the hand[25] or nodding.[26] Occasionally they consisted in vague organic and kinaesthetic sensations which the observers characterized in such terms as 'welling up' (*D,* Deral, Card 8, affirmative tendency, p. 78).

3. *The Behavior in Consciousness of Regions of the Figure where Essential Features had Formerly Occurred but were now Absent: Facile Negation. a. Normal:* When a figure of the classification-series lacked one of the essential features, or possessed it in a crucially dissimilar form, the attention was arrested at that region of the figure in which the feature should have been present. The altered region became unduly clear, and sometimes additional imaginal contents appeared. The arresting of the attention was sometimes not only described, but was also labelled by the observers as an awareness that the feature was different or 'wrong,' and sometimes it was labelled as an experience of unfamiliarity. At other times it was merely described, and not interpreted. In its present experimental setting it constituted a negative answer to the question (implicitly present as the characteristic behavior of attention) of 'does it have the essential features?' Such a halting of the attention was usually followed immediately by a negative response and by an abrupt termination of the attention-course. The observers often reacted to such an experience of 'something different,' without analyzing in detail the nature of the difference, or representing it to themselves.[27]

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[24] *C,* Deral, Card 1, p. 74, top. *D,* Zalof, Card 5, p. 75; Card 7, p. 76; Deral, Card 11, p. 78; Card 12, p. 78. *B,* Zalof, Card 17, p. 68; Tefoq, Card 6, p. 70.

[25] *B,* Zalof, Card 2, p. 66. *C,* Deral, Card 1, p. 74 (near bottom).

[26] *C,* Zalof, Card 8, p. 73; Card 7, p. 72; Deral, Card 1, p. 74.

[27] *A,* Zalof, Card 3, p. 62; Tefoq, Cards 4 and 11, pp. 64f; Kareg, Card 8, p. 65. *B,* Zalof, Cards 3, 9, 12, 17, pp. 66f, 69. *D,* Zalof, Card 8, p. 77; Deral, Card 8, p. 78. *C,* Zalof, Cards 9 and 13, p. 73.

In relatively few cases observers reported that the arrest of the attention by the altered essential feature was marked by more or less intensive kinaestheses and organic sensations, one observer even mentioning a peculiar feeling of being suspended in the air. (*B,* Zalof, Card 13, p. 67; Tefoq, Card 7, attention to curves, p. 70.) The imagery of the original series, when it occurred, contained the altered or absent feature in prominent or even in isolated fashion. This was occasionally verbal imagery of the name of the feature in question;[28] often it was concrete visual, in which the feature was either dominant or present alone, *i. e.,* the imagery was fragmentary. In the latter case, the fragment of visual imagery was sometimes projected into its proper place in the stimulus-figure.[29] When reinforcing imagery occurred, if verbal, it either consisted in the designation of the absent feature preceded by 'no,' or else it characterized the nature of the change.[30] If the reinforcing imagery were kinaesthetic, of internal imitation, it was usually accompanied by discomfort or unpleasantness, such as would be experienced if the observer normally possessed the bodily attitude of the correct figure, but was now forced to take the distorted attitude of the stimulus.[31]

b. Cautious Facile Negation: In certain of their negative classifications, the observers proceeded in a relatively painstaking fashion; if the absence or crucially altered nature of an essential feature were noted before the course of the observation was completed, the observer inhibited his imagery of 'no' or his kinaesthesis of negation and continued his exploration of the figure.[32]

4. *The Negative Response.* The arresting of the course of the main process of classifying, constituting as it did the negative answer to the questioning course of attention—'does it have the essential features?'—was usually followed only by a turning of attention away from the stimulus with the voicing of a negative response.[33]

The actual negative response, however—the process of rejecting the figure—was sometimes more complex than this mere vocalizing of the 'no.' Observers sometimes reported that their spoken "no"

[28] *B,* Zalof, Card 6, 'bifurcation,' p. 66; Tefoq, Card 5, 'hi—,' p. 69. *E,* Zalof, Card 4, p. 79.

[29] *B,* Zalof, Cards 6 and 17, pp. 66, 68. *C,* Kareg, Card 8, p. 73.

[30] *B,* Zalof, Card 6, p. 66; Deral, Card 1, pp. 70f. *E,* Zalof, Cards 4 and 13, pp. 79, 81; Tefoq, Card 7, p. 83. *C,* Zalof, Card 4, p. 72.

[31] *C,* Zalof, Card 4, p. 72; Kareg, Card 2, p. 73. For kinaesthesis of eye-movement. *B,* Zalof, Card 4, p. 66.

[32] *A,* Zalof, Card 5, p. 63. *B,* Zalof, Cards 9, 15, and 18, pp. 67, 68, 69; Deral, Card 1, pp. 70f. *C,* Zalof, Card 3, p. 72. *D,* Deral, Card 4, p. 77. *E,* Zalof, Cards 3 and 6, pp. 79f; Deral, Card 3, p. 81.

[33] *A,* Zalof, Card 3, p. 62; Tefoq, Cards 4 and 11, pp. 64f; Kareg, Cards 4 and 8, p. 65. *B,* Zalof, Cards 3, 12, and 17, pp. 66 ff; Tefoq, Cards 4 and 5, p. 69. *C,* Zalof, Card 9, p. 73. *D,* Zalof, Card 8, p. 77; Deral, Card 8, p. 78. *E,* Zalof, Cards 4 and 6, pp. 79f.

was preceded by verbal imagery, sometimes of 'no'[34] and sometimes of words which expressed the reason for rejecting the figure.[35] In a few instances an observer mentioned a slowly-increasing vocal kinaesthesis of 'no,' which culminated in the spoken negation (*C*, Zalof, Card 4, p. 72). Again, the observers occasionally reported more emphatic rejections of the figure which were marked by the presence of varying amounts of kinaesthetic and affective content, and which were sometimes closely bound up with attitudes of 'belligerency' or of aesthetic disapproval of the figure.[36]

5. *Behavior in Consciousness of Indefinite Essentials: Hesitant or Uncertain Classifications.* Two sorts of hesitant or uncertain reactions occurred in our experiments: *a.* Those in which the observers hesitated upon a region in the stimulus-figure in which a certain essential feature was neither definitely present nor absent; and *b.* those in which the observers were uncertain as to the validity of their definition of the group, and were unable to remember whether a certain feature was or was not general. The former type of hesitant reaction was by far the more common. *a.* It occasionally happened that a feature which an observer had found to be essential was altered in a striking fashion, yet not so much so as to be definitely present or absent. When this was the case, the feature behaved in a characteristic fashion in consciousness; its mode of behavior was markedly different from the behavior of definitely present or absent essential features. All of the observers reported that when their regard passed to the altered region they attended more or less persistently to the altered parts. The region thus assumed an unusual degree of clearness. In many instances an altered non-essential feature or a striking variable one behaved in the same fashion.

The arrest of the attention and its subsequent compelling and prolonged holding by the altered essential or striking non-essential was usually followed by a reappearance of the main process of classifying, but in a modified form. The observers now finished their exploration in a highly deliberate and cautious fashion; they not infrequently interpreted this behavior as a desire to exclude the figure upon some basis other than that of the indefinite feature itself. Provided no other changed aspect claimed attention, however, the observer's regard usually returned to the indefinite region, and he

[34] *B*, Zalof, Card 9, pp. 66f. *C*, Zalof, Card 13, p. 73. *E*, Zalof, Card 3, p. 79.

[35] *B*, Zalof, Card 6, p. 66. *C*, Zalof, Card 6, p. 72. *E*, Tefoq, Card 7, p. 83.

[36] *B*, Zalof, Card 2, initial negative tendency, p. 68; Card 4, p. 66; Tefoq, Card 7, p. 70. *E*, Deral, Card 3, p. 81.

ultimately responded with an affirmative or a negative judgment, or in rare instances with a doubtful judgment. The response was affirmative in eighty-two per cent of our cases.

The arrest and prolonged holding of the attention by the altered region was usually marked, sooner or later in its course, by the presence of kinaesthetic and organic, or affective, contents, or all. The observers sometimes specified tensions, strains, frowning, and the like.[37]. Sometimes the hesitation upon a feature was characterized by verbal imagery of questioning the nature of the indefinite aspect, or of fragments of the definition (*E*, Tefoq, Card 1, p. 82). Or this hesitation was occasionally marked by alternating affirmative and negative tendencies as the eye distinguished one aspect after another (*A*, Deral, Card 3, p. 63; *C*, Deral, Card 1, p. 74). In many instances imagery of the generalization-series, or imagery which supplemented the altered region in the stimulus itself, or both, occurred. The supplementary imagery was in all cases verbal. Sometimes it took the form of a more or less sketchily imaged argument, or of a silent vocalization of the aspects of the feature, as these stood out one after another;[38] again, it took the form of comments or judgments concerning the feature.[39] The imagery of the original series was visual, and the observer attended alternately to the region of the changed feature in the percept and the image.[40]

b. Occasionally an uncertain or hesitant reaction was marked by an observer's dissatisfaction with his definition, or by his inability to remember whether or not a feature had proved to be essential to the group. Dissatisfaction with the definition consisted in an attending away from the stimulus to imaged fragments of the definition—vocal-kinaesthetic or auditory verbal—accompanied by kinaesthetic and affective components, or by verbal images which expressed the nature of the inadequacy.[41] Inability to remember the exact nature of an essential feature, or to recall whether a feature had been found to be essential, consisted sometimes in the appearance of obscure consciousnesses of doubt and uncertainty, or of

[37] *A*, Deral, Card 2, p. 64; Tefoq, Card 13, attention to the central part, p. 65. *B*, Zalof, Cards 10, 2, and 5, pp. 67f; Tefoq, Card 1, attention to background, p. 69; Cards 10 and 11, pp. 70; Deral, Card 2, p. 71. *D*, Zalof, Cards 2 and 9, pp. 76f; Deral, Cards 1, 13, and 18, pp. 77, 78f; Kareg, Card 3, p. 79. *E*, Zalof, Cards 8, 10, and 14, pp. 80f; Deral, Cards 9, 4a, and 18, pp. 80f; Tefoq, Cards 1, 4, and 10, pp. 82 ff.

[38] *B*, Zalof, Card 5, 'bifur—,' p. 68; Deral, Card 2, p. 71. *D*, Deral, Card 1, attention to gray, p. 77. *E*, Deral, Cards 9, 4a, and 18, pp. 81f; Kareg, Card 4, p. 84; Tefoq, Card 10, p. 83.

[39] *E*, Zalof, Card 8, 'not similar,' p. 80; 14, p. 80; Tefoq, Card 1, 'violet bodies,' pp. 82f; 10, p. 83.

[40] *A*, Deral, Card 2, p. 64; Tefoq, Card 13, p. 65. *B*, Zalof, Cards 10 and 2, pp. 67f; Tefoq, Card 1, attention to background, p. 69; Card 10, p. 70. *D*, Zalof, Card 9, p. 76; Deral, Card 13, p. 78. *E*, Zalof, Cards 8 and 10, p. 80.

[41] *C*, Zalof, Card 1, p. 71. *E*, Deral, Card 4a, p. 82, Card 18, p. 82.

(in one case) non-bodily tensions, which were dominantly affective in character; the observer at the time attended to the doubtful feature, as it appeared in the stimulus (*D,* Zalof, Card 2, p. 76; Deral, Card 17, p. 78). At other times this inability consisted in attending away from the present stimulus to remembrances of the generalization-series, with kinaesthetic contents of questioning whether the feature were essential (*D,* Deral, Card 5, p. 77).

6. *The Behavior in Consciousness of Strikingly Dissimilar Non-general Features, or Conspicuous Novel Features.* It not infrequently happened that features which had been established as non-essential to the group attracted the observers' attention to a greater or less extent. Conspicuous novelties, or strikingly altered non-essential features received all degrees of attention. They were sometimes ignored; again, they stood out with all the clearness and persistence of altered essentials and were actually followed by a spoken negative response, although of course the alteration which claimed the attention did not afford logical justification for rejecting the figure. Many stages were represented between these two extremes, *a.* The variable feature was sometimes non-focally present to attention; here the observers noted its presence as an insignificant fact; it had no influence upon the course of the observation, nor did it arouse a reaction tendency.[42] *b.* Again, the attending to striking variables constituted a distraction from the course of observing the general features, which last was presently resumed. The variable was sometimes present in a high degree of clearness, and was even named, but had no effect upon the classification other than to delay its course.[43] *c.* Rarely, an observer reported that attention was withdrawn with difficulty from the compelling non-essential (*C,* Deral, Card 2, pp. 74f). *d.* Again, the standing-out of the non-general feature was followed by verbal imagery which expressed the non-essential character of the feature, or by verbal imagery of parts of the definition in which it had been stated that the group-figures proper might vary in respect to the feature.[44] *e.* In the majority of cases, however, the standing-out of the conspicuous non-essential feature was followed

[42] *B,* Zalof, Card 2, the bigness, p. 66; Tefoq, Card 3, the outline, p. 69. *D,* Zalof, Card 10, awareness of irregularity, p. 76; Card 2, awareness of redness, p. 76. At the time of *D*'s considerations of the 'hairs' and 'cilia' of the Deral figures he had not yet established these features as non-general in his examinations of the original series.

[43] *A,* Zalof, Card 11, p. 63. *C,* Zalof, Card 6, attending to 'dots,' p. 72. *E,* Deral, Card 2, 'redness,' p. 82.

[44] *E,* Zalof, Card 17, redness, p. 81; Deral, Card 7, size, p. 81.

by a more or less strong negative response which nevertheless did not actually overcome the course of the observation and thus lead to a spoken rejection. Any or all of the structural components which characterized the negative tendency or hesitant judgment (*Cf.* pp. 93 ff.) were sometimes present, and the whole experience was frequently followed by an unusually deliberate and thoroughgoing examination of the other regions of the figure, or by a strengthening of a negative response already present. Apparently more than the usual confirmation was necessary here in order to set free the affirmative response.[45] It frequently happened that imagery of a visual or verbal sort appeared, in terms of which the observers became aware of the possible variable character of the feature under observation. These two modalities of imagery obviously functioned in a strikingly similar fashion.[46] *f.* And the highest degree of attention to variable features is exemplified in those cases where the standing-out of the nonessential feature was followed by an actual negative response (*A,* Tefoq, Card 11, p. 64).

7. *The Experiences of Familiarity and of Unfamiliarity.* Some of the observers occasionally reported that they were aware that a feature was new or unfamiliar or that it was familiar. They sometimes spoke of a 'feeling of familiarity,' or merely of 'familiarity.' They were usually able to analyze these consciousnesses; and they found them to consist essentially in a peculiar form of behavior of the novel or the familiar feature in consciousness. The experience of unfamiliarity or of newness invariably consisted in the fact that the course of attention was arrested, or blocked, by a feature which was dissimilar to the form that it had been observed to possess in the original series; and the feature thus attained a relatively higher and more prolonged clearness.[47] Sometimes kinaesthetic components —respiratory, or manual, of pointing,—and affective components appeared in addition to the characteristic behavior of the features in consciousness.[48] The experience of familiarity proved, for *A* at least, to be more difficult of analysis. With *C,* this experience consisted essentially in the rapidity and readiness with which a feature stood out in her perception (Deral, Card 5, p. 75); sometimes marked kinaesthetic contents (respiratory changes and internal imitations) and pleasantness were present in addition (Deral, Card 1, p. 74, near bottom).[49]

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[45] *B,* Tefoq, Card 6, bigness of outline and smallness of stairs, p. 70; Zalof, Card 5, p. 68; Deral, Card 2, redness, p. 71. *A,* Zalof, Card 5, p. 63; Deral, Card 2, p. 64; Tefoq, Card 1, p. 64.

[46] *B,* Tefoq, Card 1, p. 69; Card 10, p. 70. *E,* Tefoq, Card 1, p. 83; Card 5, p. 83; Card 10, p. 83.

[47] *A,* Kareg, Cards 1 and 4, p. 65.

[48] *C,* Zalof, Card 6, 'dots,' p. 72. *B,* Zalof, Card 2, 'strange new feeling,' p. 68.

[49] There seems little reason for doubting that in *A*'s case also the experience of familiarity consisted in the fact that the familiar feature stood out readily and rapidly, and did not interfere with the course

Hence it appears that the experiences of familiarity and of unfamiliarity consisted essentially in peculiar modes of behavior of certain features in consciousness, with or without additional kinaesthetic and affective components. In all cases in which the labels of 'familiarity' or 'novelty' were applied by the observers to such experiences, moreover, our introspections indicate the presence of contents and factors in virtue of which the experience was striking and focal, a condition which evidently favored its reflective interpretation or labelling. These contents included intensive kinaesthesis[50] and prolonged duration of the blocking of attention.[51] Moreover, the presence immediately before the experience of one of the opposite sort favored its being labelled.[52]

Nevertheless, many experiences whose conditions were similar to those just described were not interpreted as familiarity or unfamiliarity, but were instead merely described, or described and labeled as acceptance or rejection of the figure, as awareness that something was wrong, as belligerence or hostility toward the figure, etc.[53] In other words, the remarkable fact appears that exactly the same sort of experiences were sometimes labeled as familiarity or unfamiliarity, as acceptance or rejection, as a yes-tendency, as an awareness that something was wrong, or indeed were not labeled at all.

Our experimental data are not sufficiently numerous to indicate in any complete fashion the conditions under which these several labels were employed by the observers. But a reference to the experimental situation throws much light upon the matter. The observers were in an 'Is-it-a-Zalof?' situation; the attention-coursing which composed the process of classifying constituted in itself the question, 'Does it have the essentials?'; and the experiences of ease and readiness of observation, or of arrest of attention, normally shifted without more ado into a 'yes-ness' or 'no-ness,' *i. e.*, they *meant* an acceptance or rejection of the figure. Consequently the observers would ordinarily merely describe the experience, without labeling it, or if for any reason they did reflect upon it or label it in retrospect, the

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of attention. In the few classifications in which he reported that a feature seemed familiar, his noting of this feature was characterized by ease of observation and by non-interference with the course of attention. The experience of familiarity was usually much less eventful and striking than that of unfamiliarity, a fact which readily explains the greater difficulty which *A* experienced in analyzing it. *A* was more successful in analyzing his familiarity during his examinings of the figures of the group proper; and his analysis here supports our present contention: *Cf. op. cit.* p. 57, foot-notes 57, p. 94, and 63, p. 96.

[50] *B,* Zalof, Card 2, p. 68. *C,* Zalof, Card 6, p. 72; Deral, Card 1, p. 74, near bottom.

[51] *A,* Kareg, Cards 1 and 4, p. 65.

[52] *A,* Deral, Card 2, familiarity which occurred after the blocking of attention by the redness, p. 64. *C,* Deral, Card 1, recognition after the initial negative tendency, p. 74, near bottom; Card 5, *cf.* doubt in connection with the previous figure, p. 75. In one introspection, at least, there is evidence that the same holds for the experience of certainty: *Cf. B,* Zalof, Card 10, p. 67. This certainty followed upon a previous doubt.

[53] *A,* Tefoq, Card 13, p. 65. *B,* Zalof, Card 4, p. 66; Tefoq, Cards 1, 6, 7, and 11, pp. 69f. *C,* Zalof, Card 13, p. 73; Kareg, Card 2, p. 73.

label favored would be that of acceptance or rejection of the figure, or of a feeling that it did or did not belong to the group. The observers were not in a recognizing situation,—the problem was not 'have you seen this figure?'—and hence the label of 'familiarity' or 'novelty' was here more remote. Consequently it is not surprising that the observers relatively seldom mentioned familiarity or awareness of novelty. The situation, then, was undoubtedly the most important factor in the interpretation or labelling of the experiences of fluency, or of difficulty and arrest of attention. Nevertheless the fact that the observers sometimes employed labels that were less directly in accord with the favored one indicates that other factors co-operated. These probably included such factors as the striking character of the components, and individual interpretation-tendencies.

D. The Relation Between the Main Process and the Affirmative or Negative Response. The preceding sections have indicated that two distinct sorts of process were operative in our classification-experiments. One of these was the characteristic course of the classifying as a whole, *i. e.*, the well-marked and unmistakable passing of attention and regard in successive fashion to the regions of figures which had been found to contain essential features. The other process was the characteristic response to the behavior in consciousness of any one feature—the affirmative response to the rapid and easy flashing out of the feature, the negative response to the sharp halting and prolonged arresting of attention upon the feature. The former was more dynamic, more forward-pushing. The latter tended toward the static; it constituted in a sense a response of consciousness to the events which composed the former. When the response was not prevented from realizing itself by the immediate continuation of the process it became richer in imaginal or kinaesthetic content, and it merged into the complex 'attitudes' which were frequently labelled as acceptance or rejection of the figure, or even as familiarity or unfamiliarity, novelty.

Our experiments indicate that a peculiar balance, or interplay, existed between these two processes or directions of consciousness. Under the conditions of our experiments, the uninterrupted and easy continuance of the process at any point in its course was endowed with a strong affirmative response-tendency; at the outset, however, this fused with the main process, and retarded the latter only slightly, if at all, *i. e.*, the continuum of the examination of the figure was straightforward, practically no approximations toward the response-state occurring. Soon, however, the response began to realize itself in more energetic fashion, and at times actually consisted in a spoken judgment, with momentary banishing of the process of classifying. (*Cf.* premature affirmations.)

Nevertheless, the classifying-process, when thus interrupted, always became reinstated upon a later occasion.

The negative response which attached to the blocking or arrest of attention upon any feature, when not immediately verbalized, usually constituted the condition of a marked retarding and prolonging of the process of classifying, when the latter became reinstated. The negative response was not confined to those arrests of attention which occurred in the normal course of the classifying-process; it also attached to arrests which were occasioned by striking variables. In other words, the negative significance which theoretically should have attached only to arrests of the classifying-process, *i. e.*, to altered essential features, also attached in some degree to striking non-essential features as well. Moreover the negative response-tendency even under these conditions was followed by a slowing of the classifying-process, when the latter became reinstated. But the negative response-tendency, when it occurred under these conditions, seldom became verbalized; its occurrence outside of the normal attention-route apparently meant a diminution of its energy, or capacity for full realization. Thus the main classifying-process and the negative response, while to some extent independent, nevertheless possessed neural bases which exerted a profound influence upon one another.

E. Individual Differences. 1. *Structural.* The structural differences among our observers had to do with the nature of the imaginal and sensory and affective components which supplemented the observers' classifications of the stimulus-figures. These components embraced five types: *a.* Imagery which presented the figures of the group proper ('group-imagery'); *b.* Contents which reinforced the observers' perceptual findings; *c.* Contents which entered into experiences of doubt or of hesitation; *d.* Personal reactions to the figures; and *e.* Contents which accompanied experiences of affirmation or of negation.

a. Group-imagery: The imagery which presented the figures of the group proper was concrete and verbal. The observers differed both as to the relative dominance of the two sorts of imagery, and as to the nature of the concrete imagery itself.

A is the only observer whose group-imagery was exclusively of one type; in his case, such imagery was concrete visual in every instance. Usually it consisted of several images of members of the original series;[54] sometimes it consisted in a single such image (Zalof, Card 7, p. 62). And rarely, his group-imagery consisted of a single schematic visual image (Tefoq, Card 13, p. 65). When *A*'s group-imagery consisted of a single particular visual image, this presented

[54] *A*, Deral, Cards 2 and 4, p. 64; Tefoq, Card 1, p. 64.

a member which resembled the stimulus-figure—sometimes one which possessed certain variable features which were similar to the variables in the present figure.

B, *D*, and *E* reported that their imagery of the original group was visual or verbal, or both. *B*'s visual images were probably the most clear and distinct of those of any of our observers. They were sometimes definite and complete and particular, of certain members of the series;[55] at other times they were semi-particular.[56] These definite and complete particular or semi-particular images were usually similar to the stimulus in form and size and in the possession of certain individual variations; and it often happened that *B*'s hesitation over a feature was terminated by the appearance of an image of a member of the series which contained a similar feature. Sometimes, on the other hand, *B*'s visual group-images were fragmentary, of parts of figures. When she became aware that an essential feature was missing, she often projected a visual image of this feature into its proper place in the stimulus-card. The fragmentary visual images were particular in nature.[57] *B*'s verbal series-presentations occurred for the most part when she noted the absence of an essential feature, or a change in such a feature; and they were usually preceded by imagery of 'no.' The verbal images were for the most part auditory,[58] but sometimes they were also vocal-kinaesthetic, when they were frequently vague and fragmentary (Tefoq, Card 5, p. 69). Moreover, the verbal images often possessed definite inflections—questioning (Zalof, Card 17, p. 68) and the like. *B*'s verbal group-imagery functioned upon rare occasions as an intention to note certain features (Tefoq, Card 6, 'green wash,' p. 70).

D's imagery of the original group, when visual, represented varying degrees of clearness and completeness and distinctness; but it was definite in the possession of the features which he had discovered to be essential to the group, and particularly of the features to which he was at the time attending. In all but one instance *D*'s visual imagery presented particular figures—a single one, or a number of them; and the images either presented members of the original series whose form most closely approximated that of the stimulus (Zalof, Card 3, Card 5, p. 75) or else they included the extremes, in form and size, of the group (Zalof, Card 7, p. 76). *D*'s verbal imagery included words which characterized the essential features of the group, and which he had used, during his examinations of the members, to characterize features whose generality he was investigating (Deral, Card 1, p. 77). In some cases D reported verbal imagery which took the form of a question regarding the nature of the original group (Deral, Card 5, p. 77). Occasionally such a verbal image functioned as an intention to investigate a certain part of the figure.

E's concrete visual group-imagery was sometimes particular or semi-particular—of definite members of the group. When this was the case, the imaged figure was almost always one which approximated the stimulus in size and form (Zalof, Cards 3, 7, 10, pp. 79f). Again *E*'s visual images were fragmentary, of parts of

[55] Zalof, Card 10, p. 67; Card 2, p. 68; Tefoq, Card 1, p. 69; Deral, Card 1, p. 70.

[56] Zalof, Card 15, p. 68; Tefoq, Card 5, p. 69; Card 10, p. 70.

[57] Zalof, Card 18, p. 60; Card 13, p. 67; Tefoq, Card 5, p. 69.

[58] Zalof, Card 6, 'bifurcation,' p. 66; Card 15, 'bifurcations,' p. 68; Card 17, auditory imagery of definition, p. 69; Tefoq, Card 10, p. 70.

the original figures which corresponded to the regions now being investigated in the stimulus (Zalof, Card 2, p. 79; Card 8, p. 80). His visual imagery was inclined to be vague and rather indistinct in all but the grossest features. *E*'s verbal imagery was usually vocal-kinaesthetic, but sometimes auditory as well; and it was remarkable for its completeness and distinctness and frequency. It is difficult to distinguish his verbal group-imagery from his verbal imagery of supplementation of his findings. In some instances, however, the former appeared before he had noted whether the designated feature was present in the stimulus; and it then functioned as self-instruction to look for that feature (Zalof, Card 6, 'see nucleus,' p. 80). Again, they consisted in parts of the definition, usually expressing the variable character of a feature being observed in the stimulus (Zalof, Card 17, 'not red—doesn't need to be,' p. 81. Deral, Cards 4a and 18, p. 82).

C's presentations of the original group-members included the widest range of image-modalities which was exhibited by any of our observers. Her group imagery was concrete visual and kinaesthetic of internal imitation, as well as verbal. *C*'s concrete visual images varied in distinctness and completeness; they usually presented whole figures, and were sometimes particular or semi-particular,[59] and sometimes fragmentary (Zalof, Card 7, p. 73). At times *C*'s kinaesthetic and organic contents of internal imitation consisted in strains and tensions and organic sensations of being posed in an attitude similar to the main form of the group members; they consisted in revivals of similar contents which had been present during the observer's examination of these figures.[60] Her verbal imagery was visual (Zalof, Card 4, p. 72) or vocal-kinaesthetic.[61] It included parts of her definition, or words which referred to various features of the original group; or sometimes the words accompanied a visual image and expressed its generality, or typicality of the series (Kareg, Card 8, pp. 73f).

b. Contents which reinforced the observers' perceptual findings: All of the observers excepting *A* occasionally reported the presence of imagery which reinforced their findings in the stimulus which was being observed; and they differed both in the amount and in the nature of this imagery.

D's and *E*'s imagery of their findings was exclusively verbal. In *E*'s case this imagery included for the most part images of words which had been used during the examination of the group to designate features which were being investigated, and it now appeared as a confirmation of the presence of these same features in the classification-stimulus (Zalof, Card 2, p. 79, etc.). Very frequently, however, the words referred to variations which were peculiar to the present figure (Zalof, Card 4, p. 79; Deral, Card 3, p. 81, etc.). Again, *E*'s verbal images referred to the similarity or dissimilarity of the stimulus to the original figures (Zalof, Card 8, p. 80). *D*'s verbal imagery designated for the most part certain peculiarities of the perceived stimulus (Zalof, Card 5, p. 76.

B's reinforcing imagery was rather infrequent; it was usually verbal, auditory or vocal-motor (Zalof, Card 12, p. 67; Deral, Card 2, p. 71; etc.) and sometimes kinaesthetic, of eye-movement (Zalof, Card 1, p. 66). *C*'s imagery of her findings in the classification-

[59] Zalof, Card 7, p. 72; Deral, Card 1, p. 74 (near bottom); Kareg, Card 8, p. 73.

[60] Zalof, Card 1, p. 71; Card 2, p. 72; Card 4, p. 72.

[61] Zalof, Card 1, p. 71; Card 4, p. 72; Deral, Card 1, p. 74.

figures was widely varied in nature,—more so than that of any other observer. It was vocal-kinaesthetic or visual verbal, the words either representing features which were peculiar to the stimulus (Zalof, Cards 3, 6, p. 72) or else designating the resemblance or lack of resemblance of the stimulus to the group (Kareg, Card 2, p. 73). Occasionally *C*'s supplementary stimulus imagery (or sensory content) was kinaesthetic, of eye-movements (Zalof, Card 3, p. 72; Deral, Card 2, p. 75). But far most striking and characteristic were her kinaesthetic and organic contents of internal imitation of the stimulus; she felt herself as posed—extended or constricted—in an attitude which resembled the main lines and directions of the stimulus figure; and if these differed from those of the group figures, she usually reported an empathic element of the discomfort—lack of balance, or strain, or dizziness—which would ensue from being in a position similar to that of the stimulus. In other words, the imitated position of the original figures was apparently a relatively comfortable one, *i. e.,* no unpleasant strain or discomfort was reported. But the distorted positions of some of the classification-figures occasioned the same discomfort, with her imitation of them, which she might feel if forced to maintain such a distorted position; and this discomfort, or feeling of lack of balance, was endowed in the experimental situation with a strong negative significance. *Cf.* Zalof, Card 4. p. 72; Kareg, Card 2, p. 73; Deral, Card 1, p. 74 (near bottom); Card 2, p. 75.

c. Contents which entered into experience of doubt or of hesitation: The observers reported in varying degree the presence of experiences of doubt, or of hesitating on a feature.

These experiences were exceedingly rare in the case of *C,* who was one of the most rapid classifiers, and who reported fewest cautious classifications (*cf.* p. 109); and with both *C* and *A,* experiences of doubt and hesitation were nothing more than the behavior in consciousness of the doubtful feature,—the manner in which it was present, and (with *A*) the cautious investigation which followed, and the tendency for the attention to return to the doubtful region. *B* reported more or less intensive kinaestheses in addition,—catchings of the breath (Zalof, Card 1, p. 66) and unpleasantness, with tensions about the chest, eyes, forehead, etc. (Tefoq, Card 11, p. 70). In the cases of *D* and *E,* the components which emerged in experiences of doubt or hesitation were kinaesthetic and organic and affective, or all three. *D* frequently reported *Bewusstseinslagen* of doubt and hesitation, which were essentially organic and kinaesthetic and unpleasant—largely the latter—and which were very difficult to localize (Zalof, Card 2, p. 76). Once he reported a 'non-bodily tension' which he was unable to describe more fully (Deral, Card 17, p. 78), and once he reported a 'mental paralysis'—a very intensive tension (Deral, Card 18, p. 79). *D*'s hesitation was sometimes dominantly verbal in content—vocal-kinaesthetic contents of questioning whether a certain feature was present. *E* usually reported that his doubt regarding a feature was characterized by kinaesthesis of close observation—strain about the eyes and brows—and by verbal imagery which expressed the nature of his difficulty: *Cf.* Deral, Card 9, p. 81; Card 4a, p. 82; Card 17, p. 82; Card 18, p. 82; Tefoq, Card 1, p. 83; Card 10, p. 83; Kareg, Card 4, p. 84.

d. Personal reactions to the figures: *B* and *E* occasionally reported certain experiences which may perhaps be called personal reactions toward a figure. In *E*'s case, these reactions were aesthetic; they consisted in the fact that a figure was pleasing or disagreeable to

him, or that it appeared ugly. The reactions were either entirely affective, or affective and kinaesthetic—strains in forehead, face, or chest—and verbal images—'ugly,' and the like: *Cf.* Zalof, Card 10, p. 80; Deral, Card 3, p. 81; Tefoq, Card 10, p. 83. In *B*'s case the personal reactions were closely bound up with her negative attention-behavior, and she sometimes characterized them as 'hostility' or 'belligerency' toward the stimulus-figure. These states were kinaesthetic and organic and usually affective; most frequently they were bound up with the reaction-word itself, and contributed to it a peculiar explosiveness of pronunciation, or emphatic intonation. *Cf.* Zalof, Card 4, p. 66; Card 9, p. 67; Card 13, p. 67; Card 15, disgust with bifurcations, p. 68. *B*'s personal reactions toward the figures appeared only in the Zalof classifications, which came first. This fact indicates the presence of a mechanizing process, by which the reactions became more and more confined to the fundamental play of attention.

e. Contents which accompanied the experiences of affirmation or negation. All of the observers reported more or less frequently that images of 'no' or of 'yes' accompanied their affirmative or negative attention-processes, or followed immediately upon them. All of the observers excepting *A,* however, reported at times the presence of additional contents, which latter differed with different individuals. *E* occasionally reported that the negative attention-response was followed by images of words which expressed the impossibility of the questionable feature being present in the original group, or which designated the respects in which the feature was dissimilar (Tefoq, Card 3, Card 7, p. 83). *C, B,* and *D* frequently reported organic and affective components, and *C* reported on rare occasions the presence of verbal images which expressed the reason for her rejecting the figure. Her affirmative contents usually consisted in kinaesthesis of nodding (Zalof, Cards 7 and 8, pp. 72f) and sometimes in a wave of the hand as well (Deral, Card 1, p. 74, near bottom). The additional components in her experiences of negation appeared as vocal-motor kinaestheses which preceded the spoken 'no' (Zalof, Card 4, p. 72) or as kinaestheses of squinting or wrinkling the forehead (Zalof, Card 1, pp. 71f). *B* frequently reported that her affirmation-experience was complicated by kinaesthesis of relaxing (Zalof, Card 2, p. 68) or by pleasantness (Zalof, Card 17, p. 68). Her experience of negation was sometimes marked merely by kinaesthesis of turning away from the stimulus (Zalof, Card 9, p. 67) and sometimes by kinaesthesis of shallower breathing and intensive fixating upon the feature (Zalof, Card 2, p. 68), or of bodily rigidity (Tefoq, Card 6, p. 70). *D* frequently reported that pleasantness accompanied his experience of affirmation: Zalof, Card 5, p. 75; Card 7, p. 76; Deral, Cards 11 and 12, p. 78. In one instance he reported an organic 'welling-up' with a kinaesthesis of extending his hand (Deral, Card 8, p. 78); again, he mentioned a 'willingness to accept' a feature, which willingness was composed in part at least of affective toning. His experiences of rejection were sometimes complicated by the presence of emotional components (Deral, Card 1, p. 77) or by slight shock (Deral, Card 4, p. 77).

2. *Functional:* The functional differences among our observers had to do with *a.* the extent to which the process of classifying was supplemented by imagery of the original series; *b.* the contditions under which such imagery appeared;

c. the extent to which the classifying-process was diverted from its course by striking irrelevancies; and *d.* the relative dominance of the main process of classifying, on the one hand, and of the affirmative or negative reaction-tendencies, on the other hand.

a. The extent to which the process of classifying was supplemented by imagery of the original series: Table II indicates the extent to which imagery of the original series occurred in the classifyings of every observer (*cf.* the two

TABLE II

The total number of classifications of every type for each observer with the per cent of cases in which imagery of the original series occurred in each type of classification. The main column-headings represent the types of classifications: facile affirmative and facile negative, cautious affirmative and cautious negative, and hesitant. Of the two columns under each main caption, the left contains the total number of classifications of that type, and the right contains the per cent of that number in which imagery of the original series appeared.

	Facile				*Cautious*				*Hesitant*		*Total*	
	affirmative		*negative*		*affirmative*		*negative*					
Obs.	No.	% Im.	No.	% Im.	No.	% Im.	No.	% Im.	No.	% Im.	No.	% Im.
A	8	00	4	00	1	00	2	00	2	50	17	6
B	8	12	8	50	1	00	5	80	6	33	28	29
C	6	16	7	28	0	00	1	00	1	100	15	27
D	3	66	3	33	3	00	5	40	3	33	17	35
E	2	100	6	16	3	66	4	50	9	33	24	41
Total	27	22	28	28	8	25	17	47	21	38	101	32
A	3	66	1	00	1	00	2	00	3	100	10	50
B	0	00	0	00	0	00	1	100	1	100	2	100
C	4	25	3	33	0	00	0	00	3	33	10	30
D	0	00	5	00	3	00	2	00	8	12	18	5
E	7	00	8	12	2	00	0	00	6	00	23	4
Total	14	21	17	11	6	00	5	20	21	28	63	16
A	3	00	4	00	4	00	1	00	3	100	15	20
B	3	33	5	40	0	00	1	100	4	50	13	46
E	5	20	4	00	1	100	0	00	3	33	13	23
Total	11	20	13	15	5	20	2	50	10	60	41	29
A	6	40	3	33	0	00	0	00	0	00	9	33
C	1	00	0	00	0	00	2	50	0	00	3	33
D	0	00	2	00	2	00	1	00	2	00	7	00
E	4	00	2	00	0	00	0	00	2	50	8	12
Total	11	20	7	14	2	00	3	33	4	25	27	18
A	20	20	12	8	6	00	5	00	8	88	51	23
B	11	20	13	45	1	00	7	85	11	45	43	44
C	11	20	10	30	0	00	3	33	4	50	28	29
D	3	66	10	10	8	00	8	25	13	15	42	16
E	18	16	20	10	6	50	2	50	20	25	68	22
Total	63	20	65	20	21	14	27	40	56	37	232	26

columns on the right). From these columns, it appears that the Zalof classifications (which were first chronologically) and the Tefoq classifications (which figures were most complex) were more extensively characterized by imagery of the original series than were the Deral and Kareg figures. Apart from this fact, however, the observers showed marked differences in the amount of such imagery in their classifications both of the same and of different groups. In the Zalof series *D* and *E* reported relatively more series-imagery than any of the other observers; in the cases of both these observers the appearance of imagery was correlated with the number of classifications which had been made (*cf.* following paragraph). In the later series—the Deral, Tefoq, and Kareg groups,—as well as in the combined classifications of all groups—*D* and *E* reported a smaller amount of series imagery than did any of the other observers. Of these two, *E* reported more imagery than *D*, *B* and *C* reported the most profuse imagery of the group, for the most part. *A* reported less than *B* and *C*, on the one hand, and more than *D* and *E* on the other.

b. The conditions under which imagery of the group occurred: Table II indicates the per cent of the classifications of each type which were complicated by the presence of group-imagery. Table III indicates the relation between the occurrence of group-imagery and the number of classifications which the observers had made. The total number of classifications was arranged in groups of five successive classifications (the last group numbering less than five, provided five was not a multiple of the total number) and Table III contains the number of classifications of each group in which series-imagery figured.

TABLE III

Relation between the occurrence of group-imagery and the number of classifications. The Roman numerals refer to groups of five successive classifications each; the figures in the other columns indicate the number of classifications in the group which were characterized by the presence of imagery of the original series. Thus, Observer *A* reported no such imagery in his first five classifications, none in his second five, one in his third five, etc. A dash indicates that the classifications were not sufficiently numerous to include a group of the advancement specified.

	Zalof					*Deral*			*Tefoq*			*Kareg*			
	A	*B*	*C*	*D*	*E*	*A*	*D*	*E*	*A*	*B*	*E*	*A*	*C*	*D*	*E*
I..........	0	1	3	4	5	4	1	1	1	2	2	1	3	0	1
II..........	0	1	1	2	4	1	0	0	1	2	1	2	3	0	0
III..........	1	2	—	0	1	—	1	0	1	2	0	—	—	—	—
IV..........	—	4	—	0	—	—	0	0	—	—	—	—	—	—	—
V..........	—	1	—	—	—	—	—	0	—	—	—	—	—	—	—
VI..........	—	2	—	—	—	—	—	—	—	—	—	—	—	—	—

TABLE IV

This table shows the per cent of classifications in which attention was distracted in varying degree by striking non-essentials, with or without an ensuing negative response. The first column of figures contains the total number of classifications, of the series specified, furnished by each observer. The next four columns contain the per cent of instances in which occurred *1.* distraction with a negative response; *2.* distraction with no negative tendency; *3.* distraction with or without negative tendency; and *4,* non-focal-awareness of the variable feature.

	Observer	*Total number classifications*	*Per cent distraction with negative response*	*Per cent distraction with no negative tendency*	*Per cent distraction with or without negative tendency*	*Per cent non-focal awareness of variable features*
Zalof	*A*	17	6	12	18	0
	B	28	7	0	14	3.5
	C	15	0	7	7	0
	D	17	0	0	0	29
	E	24	4	0	4	0
	Total	101	4	5	9	6
Deral	*A*	10	10	10	20	0
	B	2	0	0	0	0
	C	10	0	10	10	0
	D	18	0	27	27	0
	E	23	17	9	9	0
	Total	63	8	14	8	0
Tefoq	*A*	15	53	7	63	0
	B	13	23	23	46	0
	E	13	38	15	54	0
	Total	41	39	17	58	0
Kareg	*A*	9	1	2	33	0
	C	3	0	0	0	0
	D	7	0	0	0	14
	E	8	0	50	50	0
	Total	27	4	22	26	4
Total	*A*	51	21	13	35	0
	B	43	11	11	22	1
	C	28	0	9	9	0
	D	42	0	7	7	14
	E	68	14	11	26	0

From Tables II and III, it appears that in the cases of *E* and *D*, and of *C* in the Zalof series, at least, the appearance of imagery of the original group was correlated exclusively with the number of classifications which the observer had made; while in the cases of *A* and of *B*, the profusion of series-imagery depended to a large extent upon the type of classification. In *A*'s case, group-imagery was by far the most frequent with hesitant classifications, while in *B*'s case

it was most common with negative classifications, especially of the cautious type. With *B*, the failure of an essential feature to occur was in many cases marked by the superposition of a visual image of the feature upon the altered region. *C*'s results are too few to indicate definitely whether a correlation occurred between her group-imagery, on the one hand, and type of classification—or number of classifications, on the other hand.

c. The extent to which the process of classifying was diverted from its course by the intrusion of striking variables: Table IV contains for every observer the per cent of classifications in which striking variable features were attended to, with or without a resultant negative tendency. Here again, extensive individual differences are apparent. *C* and *D* in no instance reported that a negative tendency followed upon their attending to a striking variable, and (excepting for *D*, in the Deral group) it relatively seldom happened in the case either of *C* or of *D* that such a feature even stood out prominently to attention for ever so short a time. The reader will remember, of course, that neither *C* nor *B* participated in the experiments with the Tefoq series, which was more complex than the others, and in the course of which *A*, *B*, and *E* attended much more often to striking variables than they did in the other three groups. Nevertheless, if we compare the results of *D* and *C* in these last three groups with the results in the same groups of the other observers, we find that the former—especially *C*—paid least attention to variables. *A* attended most often to variables; in his case the standing-out of a variable was usually followed by a negative tendency and sometimes (two instances) by a spoken negative response, *B* stands between *D* and *C*, on the one hand, and *A* on the other. In *E*'s case, the attracting of attention was followed by a negative tendency in a small majority of instances; in two cases, he actually responded aloud with a negation. In the case of *B*, the negative tendency followed in *ca*. fifty per cent of cases.

d. The relative dominating-power of the process of classifying, and the affirmative or negative response-tendency. Table V indicates for each observer the per cent of the total number of classifications which were cautious, on the one hand, and premature, on the other hand.

TABLE V

This table shows the percentages of classifications of each type,—premature, facile, cautious, hesitant.

Observer	*Total number of classifications*	*Per cent premature*	*Per cent facile*	*Per cent cautious*	*Per cent hesitant*
A	51	25.5	37.2	17.7	19.6
C	28	17.8	57.3	10.7	14.2
B	43	13.9	41.8	18.8	25.5
D	42	11.9	28.5	31.1	28.5
E	68	4.4	51.4	14.8	29.4

Since a premature reaction was one in which the affirmative or negative response overcame the classification-process itself, it appears that the tendency to respond was relatively most potent in the case of *A*, and least potent in the case of *E*. The descending order of the observers is: *A, C, B, D, E.* When we compare the proportion of premature reactions with the number of cautious and hesitating reactions we find that a fairly close correspondence obtains between the descending order of premature, and the ascending order of cautious, reactions. *C*, who frequently reacted in very rapid fashion, constitutes one exception; the other is *E* in whose case it was difficult to distinguish cautious from facile reactions, and hence the figures in the table are less certain.

When we compare the classification-times of the different observers, it appears that the observers in whose cases the response-tendency was most frequently prepotent over the classification-process were the most rapid classifiers. Table VI contains the classification-times of every observer, for each type of classification; and Table VII indicates the descending order of rapidity of the observers, for every type of classification. A fairly close correspondence obtains between the descending order of rapidity, and the descending order of prepotence of reaction-tendencies, as shown in Table V.

TABLE VII

Descending order of rapidity of the observers in every type of classification

Facile		*Cautious*		*Hesitant*
Aff.	*Neg.*	*Aff.*	*Neg.*	
C	*C*	*B*	*E*	*B*
A	*A*	*A*	*C*	*A*
B	*B*	*E*	*B*	*E*
D	*E*	..	*D*	*D*
E	*D*	..	*A*	*C*

TABLE VI
Classification-times

		Facile		Cautious		Hesitant
		Affirmative Sec. M.V.	*Negative* Sec. M.V.	*Affirmative* Sec. M.V.	*Negative* Sec. M.V.	Sec. M.V.
Zalof	*A*	1.48±0.52	1.28±0.40	4.40±0.00	5.90±1.70	1.30±0.10
	B	1.40±0.47	1.58±0.39	3.00±0.00	2.00±0.50	2.50±0.40
	C	0.90±0.14	1.20±0.16	0.00±0.00	0.00±0.00	0.00±0.00
	D	1.78±0.00	2.30±0.10	2.60±0.20	2.70±0.20	4.91±2.34
	E	1.25±0.00	1.62±0.55	4.00±0.00	1.60±0.00	3.17±0.59
Deral	*A*	1.60±0.20	1.00±0.00	2.20±0.00	1.90±0.10	2.90±0.30
	B	....	....	....	....	....
	C	1.50±0.35	0.90±0.10	0.00±0.00	0.00±0.00	5.90±4.00
	D	0.00±0.00	3.50±1.00	5.80±1.00	4.10±0.90	6.00±1.40
	E	2.15±0.57	2.42±0.39	2.97±0.22	0.00±0.00	5.81±2.48
Tefoq	*A*	1.30±0.30	1.80±0.30	4.00±0.80	5.00±0.00	5.90±1.80
	B	1.90±0.25	1.60±0.30	0.00±0.00	4.00±0.00	3.40±0.80
	C	1.00±0.00	0.00±0.00	0.00±0.00	2.70±0.30	0.00±0.00
	D	....	....	....	....	....
	E	5.00±0.48	2.80±1.10	8.20±0.00	0.00±0.00	7.60±3.20
Kareg	*A*	1.10±0.16	1.07±0.09	0.00±0.00	0.00±0.00	0.00±0.00
	B	....	....	....	....	....
	C	....	....	....	....	....
	D	0.00±0.00	1.00±0.00	4.60±0.60	3.00±0.00	6.60±3.40
	E	2.30±0.70	1.20±0.00	0.00±0.00	0.00±0.00	3.30±1.30
Total	*A*	1.37±0.29	1.08±0.26	3.53±0.80	4.20±0.90	3.36±0.40
	B	1.65±0.36	1.59±0.34	3.00±0.00	3.00±0.50	2.90±0.60
	C	1.13±0.24	1.05±0.13	0.00±0.00	2.70±0.30	5.90±4.00
	D	1.78±0.00	2.40±0.55	0.00±0.00	3.20±0.55	5.83±2.38
	E	2.70±0.58	2.01±0.35	4.33±0.60	1.60±0.00	4.97±1.89

F. SUMMARY. 1. The process of classifying,—the observers' manner of perceiving the classification-figures,—consisted in a series of experiences in which attention passed in order to the regions of the classification-cards whose counterparts in the group proper had contained essential features; *i. e.*, it consisted in a successive becoming focal—definite and intensive—of these regions. This course of attention persisted either until the regions of all the essential features had been examined, or until an essential feature failed to stand out when the regard passed to its region.

2. The process of classifying was sometimes complicated by the presence of components other than the perceptions of the classification-figure. These contents included vocal-kinaesthetic and auditory images of words which designated the observers' findings; kinaesthetic sensations of the eye-move-

ments involved in examining the figures; kinaestheses of internal imitation of the figures; and concrete visual and verbal imagery of the original series.

3. The process of classifying was initiated only by the hearing of the experimenter's instructions and by the perception that the figure had been exposed. When this process reinstated itself after an interruption, however, its recurrence was at times preceded by concrete visual or verbal imagery of the region of the figure which had not yet been explored; and sometimes by verbal imagery of self-instruction. It seems probable that imagery of the first type is to be regarded as an initial term of the reinstated process of classifying, rather than as imagery of intending to continue the exploration, or of setting up a goal-idea.

4. The component details of the process of classifying,—the conscious experiences that the classification-figures contained or failed to contain specific essential features, etc.,—consisted in the behavior in consciousness of the separate features of the classification-figures. This behavior depended upon the extent to which the regions of the essential features resembled the corresponding regions in the group-members. *a.* If a feature which the observer had found to be present in all of the group-figures was definitely present in the classification-figure, it flashed out into clear consciousness as attention passed to and over its region; and this facile and often very brief standing-out of the figure constituted in itself the affirmative answer to the question 'does it have the essential features?' which was present as the characteristic course of attention in the classification. It was sometimes complicated by the presence of additional contents—imagery of the original series, or contents which reinforced the observation of the figure itself, such as verbal imagery, kinaestheses of internal imitation, and the like. Again, it was often followed by a verbal affirmative response, imaged or overt. If the observation had not been completed, the attention shifted rapidly and readily on its course; and if the observer did not note the absence of any essential feature, an affirmative response was verbalized.

It sometimes happened that the affirmative response was verbalized before the course of the process of classifying was completed. Thus a premature reaction was given; the affirmative reaction-tendency overcame the process of classifying. Even here, however, the uncompleted process usually reasserted itself, operating on a last percept of the disappearing stimulus, or upon concrete visual imagery of the stimulus.

In some cases observers reported classifyings of a type in many respects the opposite of the premature one. The successive features were noted in a slower and more painstaking fashion; the affirmative response followed less readily and rapidly.

The affirmative response itself usually consisted only in the ready standing-out of the feature, and the rapidity and uncontestedness with which the attention left it; it merged without delay into the continuing classifying-process. Sometimes, however, the attention-processes were complicated by additional components, such as relaxation, pleasantness, kinaesthesis of nodding, of waving the hand, or internal 'welling-up.' Verbal imagery of 'yes' was often present; and under certain conditions—usually, when the feature was the last one on the attention-route—a spoken 'yes' followed.

b. When a figure of the classification-series lacked one of the essential features, or possessed it in a crucially dissimilar form, the attention-course was arrested at that region, which became unusually clear. This halting of the attention constituted the negative answer to the question (present as the characteristic course of attention) of 'does it have the essential features?'; and it was usually followed immediately by a negative response and an abrupt termination of the attention-course. Occasionally the arrest of attention on the altered region was accompanied by more or less intensive kinaesthetic and organic sensations. Or imagery occurred of the region concerned as it had appeared in the group-figures themselves, and of supplementing the findings in the present figure.

In some cases, however. the negative response was not verbalized aloud, but instead the course of the classifying-process continued, in a relatively painstaking fashion.

The negative response itself was usually nothing more than the above-described sharp arresting of the attention, with the subsequent verbalized 'no.' Sometimes, however, it was complicated by the presence of additional contents,—more or less emphatic and intensive kinaesthetic and organic rejectings of the figure, verbal imagery of 'no,' and of words which expressed the reason for rejecting the figure.

c. When an essential feature was markedly dissimilar, yet neither definitely present nor absent in the classification-figure, attention remained persistently upon the region in question,—the consciousness of the region lasted, and it became very focal. When after a time the exploration of the figure was continued, attention was cautious and deliberate in its course, and it frequently returned to the altered region. This pro-

longed arrest upon the altered region was usually marked, sooner or later, by the presence of strains and tensions and affective contents of unpleasantness. Imagery of other sorts also occurred on many occasions; verbal imagery of questioning the nature of the altered aspect, or of fragments of the definition, or of sketchily present argument, as one aspect after another stood out in the altered region, or of silent vocalization of these aspects; and visual imagery of the corresponding region in figures of the group proper.

5. It sometimes happened that conspicuous novelties or strikingly altered non-essential features stood out in consciousness during the examination. These features were sometimes merely present in a marginal fashion, without influencing the main course of attention. Again, they became for a time very focal, but were not followed by affirmative or negative responses. Their focal presence was sometimes followed by verbal imagery of their non-essential character, with turning of attention to other regions. Usually, their standing-out was followed by a more or less energetic negative tendency, which was at times actually verbalized aloud.

6. The observers sometimes merely described the affirmative or negative responses without labelling them. In other cases they described them and also labelled them, sometimes as familiarity or unfamiliarity of the feature in question, but usually as acceptance or rejection of the latter, feeling that it was wrong, etc. The application in retrospect of some label to the experience depended largely upon the additional presence of striking kinaesthetic and organic and affective or other contents or factors; it also depended to some extent upon the individual observer. The nature of the label which was applied depended in part upon the individual observer; but it tended to be closely harmonious with the prevailing direction or course of consciousness, in this case, the process of classifying. The observers were in an "is-it-a-Zalof?" situation, and the normal interpretation of the experiences of arrest, or of ready passing on, of attention was obviously one of rejection or of acceptance, of feeling of figure being right or wrong, etc. The observers were not in a recognizing—a "have-I-seen-it-before?"—situation and hence the infrequency of the label of familiarity or of unfamiliarity is not surprising.

7. Two main sorts of process were thus operative in our classification experiments. One of these was the characteristic course of consciousness in the classifying as a whole; and the other was the response-tendency, which appeared with the standing-out of any feature. The former was more forward-

moving, more dynamic; the latter, when it separated out from the former, tended toward the static ('conscious attitude'). It frequently failed to realize itself, however, the classifying-course continuing instead. When the response (especially the negative response) was not thus prevented from realizing itself, it became richer in imaginal and kinaesthetic content, and merged into complex attitudes, which were labelled as familiarity, acceptance, or especially unfamiliarity, rejection, and the like.

Ordinarily, the process of classifying prevailed until its completion, but at times the response-tendency became sufficiently strong to interrupt it for a longer or shorter time, the response even being verbalized aloud.

The introspections reveal the fact that a certain interdependence existed between the main process of classifying and the affirmative and negative response-tendencies. While the negative response-tendency often appeared when attention went to the region of a striking variable feature, nevertheless it relatively seldom appeared here in sufficiently energetic manner to lead to a negative spoken response. Its appearance apart from the normal route of the process of classifying apparently meant that its energy was diminished. On the other hand, the appearance of such an energetic negative tendency was followed by a checking and a becoming more cautious of the main process itself, when the latter became reinstated.

8. Individual Differences: Our observers presented numerous individual differences, both structural and functional. Structurally, they differed in the nature of the imagery which presented the members of the group proper; in the nature and modality of the imaginal reinforcement of the figures under observation; in the nature of the contents which entered into the experiences of doubt and hesitation; in the frequency and nature of personal reactions to the figures; and in the nature of the imaginal and sensory contents which accompanied experiences of affirmation and negation. The functional differences were concerned with the extent to which the process of classifying was accompanied by imagery of the group proper; with the conditions under which such imagery occurred; with the extent to which the process of classifying was diverted from its course by striking irrelevancies; and with the relative dominating power of the process of classifying, on the one hand, and of the affirmative or negative reaction-tendencies, on the other hand. With certain observers, the appearance of imagery of the original series depended upon the number of past experimental sittings with the series

in question; with others, it depended upon the type of classification, being more numerous with the hesitant or cautious negative type. In general, the observers in whose cases the response-tendency frequently overcame or delayed the main classification-process showed fewer cautious reactions; and their reaction-times were most rapid.

IV. Conclusions

The essence of the process of classifying, as this process occurred in our experiments, consisted in the manner of the observers' perceiving the object which he had been instructed to classify. This manner of perceiving consisted in the fact that the regions of essential group-features were stressed in consciousness, and that these regions behaved in consciousness in a fashion which depended upon their resemblance, or lack of resemblance, to the corresponding features in the group-members. In the former event, the regions in question passed in and out of consciousness in rapid and ready fashion, without retarding the course of attention. In the latter case, on the other hand, the course of attention was arrested sharply; these regions often persisted in consciousness, and they were frequently accompanied sooner or later by more or less focal and intensive kinaesthetic, organic, and affective contents which functioned in their conscious settings as definite rejectings of the figure.

This view is, we believe, in accord with those of numerous writers who hold that the generalizing-process is essentially a motor phenomenon. The classifying of a novel percept with a group of previously experienced ones occurs in virtue of the fact that the former arouses a reaction which is identical with that aroused by the latter.[62] Within the writer's knowledge, the authors who have most persistently concerned themselves with the conscious phase of this motor response are Zetz and Müller-Freienfels. Betz[63] finds that in his own case, the motor reaction or 'attitude' is either projected out into space, *i. e.*, objectified, as a series of movements (whether kinaesthetically or visually conscious is not fully clear to the writer); or else it forms the basis of a feeling—*e. g.*, in the case of green, a 'calm, agreeable feeling.' A concept is a 'projected' attitude; and a novel phenomenon is subsumed under a concept when the attitude which it evokes 'fits in' with the concept-attitude (pp. 210 ff). The meaning for consciousness of the expression 'fits in' is not clear to the writer. Whether it means an inference on the part of the systematizer, or an actual consciousness of 'fitting in' on the part of the experiencer, we cannot say.

Müller-Freienfels maintains that the motor reaction has a feeling as its conscious symptom; and he uses the term *Stellungnahme* as an

[62] *Op. cit.* footnote 2 p. 57, p. 24.

[63] W. Betz, Vorstellung und Einstellung. II. Ueber Begriffe. *Arch. f. d. ges. Psychol.* XX, 1911, 186-225.

expression for the combination of motor disposition and feeling-symptom.[64] Feeling is not sensation, nor is it a property of sensation; whether its essence consists in movement, he leaves open.[65] Sensory situations which arouse the same *Stellungnahme* are subsumed under the same concept. Here again, the writer is uncertain as to whether Müller-Freienfels uses the term 'same' in an inferential sense, or as referring to a consciousness of sameness.

If we attempt to translate our findings into the language of Betz and Müller-Freienfels, we would conclude that the 'motor disposition' 'set free' in the percept may be present to consciousness in terms of the manner in which the observer perceives the object, *i. e.,* the shifting parts of the latter which stand out, and the duration and intensity and focality of the standing-out of the several parts, or in terms of kinaesthesis of the observation as well.[66] We would maintain that the 'feeling' referred to may possibly be analyzable into the peculiar temporal and focality aspects of the percept which constituted its being included in the group, or into those which constituted its being rejected; and we would suggest further that the presence in addition of an awareness or attitude of sameness, or of 'fitting in,' is a function of the momentary conscious situation of the observer, and of the use which he is to make of the experience. If, *e. g.,* he is told to identify the object, his manner of perceiving it will be supplemented by verbal or kinaesthetic assent, which constitutes an awareness of sameness. If he is making a different use of the percept (*e. g.,* examining its features for specific points[67]), he will have no such awareness of its identity with the group.

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[64] R. Müller-Freienfels. Typenvorstellungen und Begriffe. *Zeitschr. f. Psychol.,* LXIV, 1913, 386-433.

[65] R. Müller-Freienfels. Die Bedeutung der motorischen Faktoren und der Gefühle für Wahrnehmung, Aufmerksamkeit, und Urteil. *Viertelj. f. wiss. Phil.,* XXXVIII, 1914, 215-253; 335-371.

[66] We believe that observers vary with respect to the manner in which such motor phenomena are present to consciousness, *i. e.,* whether they are present as shifting visual (or auditory) perception, or as kinaesthesis, or both.

[67] *Cf. op. cit.* footnote 2 p. 57, pp. 97 ff.

RESEARCH IN PATHOLOGICAL PSYCHOLOGY AND BIOCHEMISTRY [1]

By Edward Cowles, M. D.

Research in the pathology of mental disease has a history of peculiar interest; progress has been indirect and difficult in this branch of the medical sciences because of both favoring and conflicting influences. In modern psychiatry laboratory investigations were first employed to find in anatomical pathology explanations of mental disorders. Great progress has been made in the 70 years since Griesinger published his work on mental disease. During the first 20 years of that period he was the first to establish psychiatry upon the basis of scientific research and pathological principles. During the next 20 years several contributing movements, having their genesis in the advancement of general medicine, gained headway or had their inception; in America, a new interest arose in pathological investigations in institutions for the insane, and among them the plan was formed at the McLean Asylum (Hospital after 1892) to add to the pathological laboratory two other laboratories in order to combine researches in physiological and pathological psychology and biochemistry with the clinical work. In 1889, after ten years of preparatory observation, under the inspiration of general medicine and the "new psychology," these combined laboratories were fully organized and equipped for beginning researches by the methods then available.

In the laboratory movement of the time the new combination here described was the outcome of definite concurring influences, which, with their sources, should be recognized for their historical significance. The McLean Hospital laboratories reflected contemporary trends leading to the momentous changes which are now revolutionizing ideas that have long dominated psychiatry; it is important to note what these changes have contributed to a stage of progress that has been slow in awakening the attention of alienists. In the asylums, as they were constituted in the decade of 1880-90, the medical service was concerned with a number of quite distinct major problems in

[1] An account of the laboratories of the McLean Hospital. Reprinted from The Institutional Care of the Insane, 1916, Vol. 2, pp. 618-636, with added notes and references.

theory and practice; these related (1) to the methods by which explanations of mental disorder were sought in the findings of the pathological laboratory; (2) the generally accepted psychological conceptions and formulæ descriptive of mental physiology and of mental diseases for their classification; (3) the physical conditions associated with mental diseases and their causes with respect to the principles of general physiology and of general medicine; (4) the practical methods of treatment of the different forms of these diseases.

The General Position of Psychiatry in America in its Theory and Practice, 1880-90

The attitude of the alienists toward their problems was one of newly stimulated interest. Some of the difficulties met with and the obstructive effects of certain misleading contradictions in the formulations of the problems should be noted; they will appear in the following account of the McLean laboratories as an example of the general efforts toward progress. The purpose, course pursued and results accomplished can be better understood by indicating here the observations concerning the nature of the major problems that prompted the laboratory investigations. (1) The alienists were subject, by general consent, to the claims of pathological anatomy as the master science in general pathology; they looked to it for a basis of scientific psychiatry, but it was hope deferred, and they remained under the reproach of being unscientific. Although the new science of neurology, claiming psychiatry as a part of itself, was then bringing much aid, yet within 20 years thereafter it reached one of its authoritative conclusions that "pathological anatomy is of more academic than practical interest to the psychiatrist . . . the burden of our work should be away from morphology and more in physiologic lines." This of course did not deny the essential value of pathological morphology in association with these fields of investigation.

(2) Mental physiology, with true explaining principles in the physical mechanism, was the immediate need of the alienists for the investigation of abnormal behavior, and the first step in "tracing back symptoms to structural changes in accordance with the principles of general pathology." Lacking such explanations and limited to descriptive classifications of clinical symptoms, the alienists avoided the speculations of academic psychology which gave little aid. Thus, in common with all the world, they adopted the general empirical conception of the intellect, feelings and will; and in terms of these conceptions they framed their descriptions of the mental activi-

ties. Twenty years prior to the time under consideration Griesinger was making the bequest of his conceptions of mental pathology universally followed in modern psychiatry; his formulations of the symptom-complexes of melancholia and mania still dominate our descriptions of melancholia as "states of mental depression," and mania as "states of mental exaltation."

These phrases condense one of the most fundamental and general concepts of psychiatric theory into two words, "depression" and "exaltation," used as physical metaphors for the contrast of the feelings of pain and pleasure, which present a true "oppositeness" of *quality* of feeling or emotion—both normal and morbid. But while "depression" always fitted melancholy states, "exaltation" was found to be far from constant in maniacal states and not to fit the physical facts; and the make-shift word "excitement" came into use in its motor sense. To-day we frame our descriptions around "depression" and "excitement," making a false differential of emotion and activity, which always seems to imply a *quantitative* contrast of decrease and increase; whereas, on the contrary, in the states of real physical depression with constant mental pain there is always increase of its intensity and often motor agitation, while in the states of excitement—of shifting emotion and motor activity—there are further real decreases of integrity of both mental and physical functions and descent to "deeper levels of destruction." Such are some of the contradictions in this slough of despond for scientific psychiatric thinking and experiment. There is no way of escape but to abandon it. It is an inheritance from the most primitive experiences of an ever-widening range of such concepts of the feelings, as of pleasure and pain, joy and sadness, hope and fear, as expressed in such figures of speech as exaltation and depression, being uplifted or downcast, and innumerable analogues of highness and lowness in the moral sense; thus have been drawn into the complex conceptions of the feelings the physical meanings of such ambiguous words.

There can be no change in the common usage of such picturesque analogies and physical metaphors; but in the present instance they are destructive and unfit. The remedy must be by passing from facile description to the explanatory level; and to the advancement of laboratory investigations in psychology, psychiatry and physiology are due the definite signs of emergence in recent years. In 30 years, or since the beginnings of Wundt's psychophysic experiments to Pawlow's and Cannon's latest discoveries in the physiologic relations of protective bodily changes to emotional reactions, there has been a revolution of ideas through the contributions of physiology to psychiatry.[2]

[2] "For a number of years, and particularly since the publication of Pawlow's work on the effects of emotion upon glandular action, there has been a wide and increasing interest among psychologists and physiologists in the more intimate bodily mechanism underlying emotional processes. This movement has coincided with a rapidly growing

(3) The physical conditions associated with mental diseases were of constant interest to the alienists; systems of classification were attempted, based upon the etiology of mental disorders as sequences of general diseases. This tended to aid in prompting the purpose of the laboratories which grew out of the conditions observed in the preceding ten years, showing the need of physiological investigations to explain the psychological problems. Some of the fallacies of the conceptions of them have been noted in the foregoing statements to show the reasons for the attempt to apply new methods. The attitude of inquiry is indicated by the position taken in respect to Griesinger's description of contrasting mental states before quoted. Beginning in 1885, and since then carried forward in annual courses of lectures and in projecting these laboratories, the view has been presented as the belief of many alienists that melancholia and mania constitute one disease, with stages declining to dementia. It was proposed two years later, in an unpublished paper on classification, to amend Griesinger's formulations to read "states of depression of feeling" and "states of derangement of intellect," for the reasons that while the persistent mental pain characteristic of the first stage reflects truly the bodily malaise, the second stage is characterized by graver derangements of thinking and unstable emotional states, showing their disordered relations with the bodily ill-being and the losses of functional power and control. This was known to have been essentially the position taken by Griesinger, but it can be better appreciated in the light of later understanding by newly studying his writings, which reveal his vigorous but unheeded protest against the immediate misinterpretation by his contemporaries of his psychological meanings. Limiting the present view of the situation to what it was in the decade prior to 1889, it can now be seen that he used the words "depression" and "exaltation" in their figurative sense, as did the contemporary alienists, psychologists and all the world besides, following the ancient and fixed usage. But he meant simply "mental pain" and the emotional contrast; he did not mean an oppositeness of "mental torpor" and "mental irritation"—an "increase" or "decrease" of some-

appreciation among physiologists and physicians of the organic significance of certain of the so-called ductless glands, and of the physiological importance of gland and muscle tissue in general. Already the discoveries made have quite revolutionized many of the ideas of a generation ago, and the chapter seems hardly more than begun." James R. Angell, Review of Cannon's work on "Bodily Changes in Pain, Hunger, Fear and Rage. Recent Researches into the Function of Emotional Excitement;" and Crile's work on "The Nature and Origin of the Emotions." *Science,* Nov. 12, 1915.

thing. Yet the psychiatric world still clings to the ancient, unscientific conception of a physical *something, up or down,* although the physiological investigations of the laboratories have been proving for 30 years that Griesinger was right. The following narration of events will show the course of progress, and what was done with the ancient conceptions when the new psychology brought psycho-physic experiment into psychiatry.

(4) The most significant indication of the attitude of the alienists of 30 years ago toward general medicine was their position with respect to the practical methods of treatment. They had been the leaders in the adoption of the "supporting treatment" for many years in advance of general medicine—a logical, though empirical, expression of their comprehension of the physiological principles of the energy concept. Here was the basis of the vogue of the neurasthenic concept developed in the science of neurology. This was the crux of the matter from which came the inspiration to research in the chemistry of nutrition. The "fatigue question" and the "nutrition question" were "believed to be of primary importance in psychiatry." Hence as an outcome the laboratory of biochemistry. The psychological laboratory was added by direct importation from the laboratory of Ludwig at Leipsic, through Dr. Stanley Hall and the department of psychology at The Johns Hopkins University. These preliminary comments have a certain historical interest; they are also needed not only to show the origins of this laboratory movement, but to state the problems with which it had to contend, and which essentially form the basis of the conclusions that emerge from the broader review of the whole matter.

NOTE.—The main events on the medical side of the laboratory movement at its beginning. indicate the conditions under which the aid offered by psychology had to make its way. While some of the earlier alienists, usually chosen men of attainment in general medicine, gave special attention to pathology it was not until 1868 that Gray at Utica, first in this country, organized a department for such research by appointing a pathologist; this was continued during his service till 1886. Through this period there was increasing interest in such investigation by individual members of the medical staff in some of the asylums. But the most stimulating influence of the time was the work of Folsom in Massachusetts, then Secretary of the State Board of Health, which included the supervision of the care of the insane; his historical monograph on "Disease of the Mind," 1876, showing European and American progress, and his observations, especially of English and Scotch methods, aroused great interest; many of our younger alienists went to study them during the next decade.

At this time the pathologists were non-resident, appointed for occasional service. The Government Hospital in Washington made a great

step in advance by establishing a permanent department, the most efficient of the time, for scientific work with the appointment of a special pathologist in 1883. This work in clinical pathology was of the best order and was maintained until 1906 when the scope of the department was extended by the appointment of an expert psychologist experienced in pathological psychology and not a physician, to be the scientific director with various assistants. This combined service has sustained its hopeful purpose. This event is significant of the long cycles of time taken for such an innovation as that of pure psychology into psychiatry; Dr. Franz had been for four years before the first expert psychologist in such a service at McLean Hospital, where twenty years before Dr. Noyes was appointed pathologist also to direct especially the chemical research as well as to apply the psychology he had learned from Stanley Hall at Johns Hopkins University.

In the next following decade, 1890-1900, the general movement became more manifest, and resident pathologists were appointed. At Kankakee, Dr. Meyer took charge in 1893, and went to Worcester in 1896; at Danvers, Dr. Worcester was appointed in 1895; at Indianapolis a spacious pathological building was opened in 1895; and at the Sheppard and Enoch Pratt Hospital, Dr. Paton was appointed in 1899; all of these and other like efforts have established their value as important adjuncts in the progress of the clinical work. It appears in all that the initiative purpose was for scientific investigations in pathology.

This was also the essential purpose of the largest undertaking in that decade, the State Pathological Institute in New York city in 1895. The State Commission and Dr. Van Gieson, the director, had ambitious and comprehensive conceptions of the main purpose of this central influence for guiding the scientific work of all the hospitals. Progress was made in organizing the pathological work; there were interesting beginnings in the study of psychological problems and of chemical investigations. But its history shows that the disadvantages of its being detached from a clinical service and the danger of failure in its being supported in its costly location caused its removal to Ward's Island, where it could be in connection with hospital wards assigned to it, and be afforded facilities for developing a practical field for co-ordinating all the investigations which would invite attention in an economic association with a clinical service; this would obviously claim existence for its own sake. This original purpose was promoted and broadened; the way of progress opened wide under the direction of Dr. Meyer who came there from Worcester in 1902 and was succeeded in 1910 by Dr. Hoch with an experience of twelve years in the McLean laboratories and five years at Bloomingdale Hospital. The trend of progress of this great establishment is indicated by the change of name from Pathological to Psychiatric Institute. The facts accomplished in its course of twenty years need no recital here of what it has done in applied psychology. These references to the origins of the laboratory movement and the concurring events in the quarter century before 1900 indicate the circumstances contemporary with the following account of the McLean laboratories. Out of these labors of that time has come the later remarkable development of reception wards, and psychopathic hospitals tending to the provision of a psychopathic building in every hospital. The significance of all this is very great, and the appeal to psychology, the master science of psychiatry, and points to the need of trained psy-

chologists, as well as pathologists in the laboratories. Every date mentioned implies years of preparatory projects obstructed and delayed; the following history shows twelve years of active work in the combining of researches and prior years of preparation before 1900, in the McLean laboratories. Up to that time such a combination of psychology and chemistry with pathology and the clinic had not been projected elsewhere. It was a few years later there when first a trained psychologist, also a teacher of neural physiology, was persuaded to enter this special field for a professional career, in this country, and later one other followed him; these, with a few others more recently, amply justify the coming demand for this great service to psychiatry.

The Organization of the Laboratories

The new laboratories at the McLean Hospital were early attempts to combine the methods of physiological, biochemical and psychological experiment, under the principles of general medicine, in the clinical work in such institutions. The general progress made was noted in the annual reports; of special interest are those from 1888 to 1892, describing the establishment of the three laboratories, and a review of the work of the hospital in the reports for 1901 and 1902. Biological chemistry and physiological psychology were held to have an essential dependence upon each other. The conception of the chemical laboratory as a clinical aid in psychiatry had a longer incubation and was the direct expression of the neurasthenic concept, or, better, the energy concept. Here, as before noted, there was a direct sequence of events. From the time of the "supporting treatment" and Griesinger's recognition of the principle of reduction of energy in the relation between melancholia and mania, the energy concept had been gaining its place of fundamental importance in general medicine, and has held it in the practice of psychiatry, though much obscured and mostly lost to sight in theory.

The aims of the combination of the laboratories were comprehensively stated in a report on the psychological laboratories in America, contributed to the *Année Psychologique* in 1894 by Dr. Delabarre, in which the McLean Hospital psychological laboratory and its equipment were described as follows:

"The purpose of establishing and developing the laboratory has been carried on under much difficulty, naturally due to the newness of the attempt to combine with psychiatry the other departments of scientific medical research. The pathology of the terminal stages of insanity must be studied as heretofore, and it is necessary to add that of the initial conditions which lead to mental disorder. Such studies must therefore be combined with physiological psychology in the attempt to determine the exact nature and causes of departures from

normal mental function. Also in the dependence of these changes upon general physiological processes, and in order to take into account all the elements of vital activity involved, it is supremely necessary to study both physiological and pathological chemistry in their direct and indirect relations to mental changes. It will be seen by the foregoing report that the fatigue question, and its relation to auto-intoxication, is believed to be of primary importance in psychiatry. It is inevitable that progress must be slow in developing these several concurrent lines of inquiry; but the researches already begun are most interesting and promising, and encourage the hope that the work which is contemplated will so effectively combine them all as to yield worthy results."[3]

The laboratory, with its two purposes, was described by Dr. Delabarre as "the only one in America which united psychiatry and physiological psychology. In Germany there exists only one like it—that of Professor Kraepelin at Heidelberg. It attempts to combine the studies of the clinic and of neurology with those of chemistry on the one hand, and with those of psychology on the other."

In a lecture on "Neurasthenia" in 1891, and in other papers,[4] there is an extended discussion of the energy concept as it is involved in the successive reductions of functional capacity in neurasthenia, melancholia and mania, considered as stages of one disease—meaning that a *neurasthenic condition* underlies all of these phases.

The laboratories were under the direction of Dr. Noyes, appointed in 1888 and continuing till 1893, including that of pathological anatomy; and a seminary was organized by the medical staff for study in psychology and psychiatry. The equipment for psychological experiment was guided by that of Dr. Hall at Johns Hopkins University, and at Harvard Medical School, aided by the counsel of Dr. Bowditch and Prof. James; the chemical work had the valuable advice of Dr. Wood and Dr. Chittenden. The direction of the three laboratories was continued by Dr. Hoch for nearly twelve years, beginning in 1894; during nearly two years in Europe, his studies, mainly in pathology at Strasburg, and at Heidelberg with Nissl, had been extended to special preparation for the service at McLean Hospital by devoting time to the work of Kraepelin, Wundt and Mosso. The provision of ample rooms

[3] "Les Laboratoires de Psychologie en Amerique," by E. B. Delabarre, *L'Année Psychologique,* 1895; also "Laboratory of McLean Hospital," by G. Stanley Hall, *Amer. Jour. Insanity,* 1895.

[4] "Neurasthenia and its Mental Symptoms," Shattuck Lecture, E. Cowles, *Amer. Jour. Insanity,* 1891; "The Mental Symptoms of Fatigue," *Trans. N. Y. State Med. Assn.,* 1893; also "The Problem of Psychiatry in the Functional Psychoses," *Amer. Jour. Insanity,* 1905. See also "The Mechanism of Insanity," *Ibid.,* 1889-91.

and equipment for research in the new McLean Hospital, opened in 1895, was justified by the early experience. The claim of the chemical laboratory to a place in clinical psychiatry was established by the investigations of Dr. Folin, who took charge of it in 1900, when he applied the methods of pure chemistry and gave it a recognized distinction. The development of the work as a whole was aided directly by the conceptions of the English physiologists. It was noted at the outset that the clinical questions were characteristically associated with causes producing neurasthenic conditions, and with recoveries through restoration of the general health and strength. As stated later by Dr. Folin, "the problem to be dealt with in such cases is very largely that of nutrition, and the nutrition question is fundamentally a chemical one; also that it is disorders of metabolism that have a large part in the derangements of nutrition and dependent functions of the nervous system; and it is to such derangements that disorders of the mental function may be due in many cases." The purpose from the first was to approach such problems from the side of general medicine and to determine "what conclusions can be reached concerning the important question whether any tangible relation between faulty nutrition or other faulty metabolism and different forms of mental disease can be established." This implies the study of the influence of functionally disordered bodily conditions and organic sensations causing alteration of the "sense of well-being" and "personality," and of the "sense of adequacy," producing obstructive interferences with the processes of feeling, thinking and doing. The reactions of the emotions were recognized, when first undertaking the joint investigations, as essential factors of the utmost importance, in "the changes in nervous reactions in health and disease, the relation of the mental element as to its interferences with these reactions, and the counter influence of bodily conditions upon mental states." The conclusion was drawn that "in acute neurasthenia and in true melancholia and mania there is always nutritional and toxic functional weakness, fundamentally, in the organism; it is from this that the influences arise which affect the conscious feeling and thinking, making these higher mental states the sensitive indices of the lower physical changes." [5]

[5] Op. cit. Neurasthenia and its Mental Symptoms, 1891. The quotation continues: "When all goes well with the organism and it is in a condition of unfelt equilibrium, the processes of thinking and feeling are adjusted, more or less logically, to the varying environment upon a basis of a sense of well-being and normal love of life. On the other hand, a morbid process may be started in these higher

In this view it was believed that all the operations, physical and chemical, of the nervous and mental mechanism, should be studied as being conditioned by the contributing and concurring activities in both fields, and as having a primary and necessary relation to the supporting energy. The conscious attitude of the moment, having its inseparable emotional factors, affecting both the mental response and the physical reactions, these in turn must condition, and may determine, a persistent affective tone, especially when of pathological intensity. "The phenomena of nervous life are the outcome of a contest between what we may call inhibitory and exciting or augmenting forces" (Foster, Physiology). Voluntary action is at all times the resultant of the compounding of our impulsions with our inhibitions (James).

The point of present interest is that these laboratories and their combination were simply outcomes of principles then generally recognized, but awaiting effective acceptance in psychiatry. The significant exception to this was that the energy concept which, having given psychiatry the leadership as its long-used practical guide for treatment, had gained only a slow and limited appreciation of its special physiological and biochemical import for mental diseases. The result followed that psychiatry has engaged itself in working out other trends of

activities, in a previously healthy and strong organism; but until the organism itself suffers a change to the specified nutritional and functional weakness there can be no such mental symptoms as are being studied here. Normal mental activities cannot produce 'mental symptoms' except by first causing the characteristic 'weakness' somewhere in the physical basis of all of them."

It is of great historical interest in this connection that one who newly reads Griesinger's conceptions of more than 70 years ago may trace the substantial evidence throughout of their underlying continuance and growing force to the present time. The words he wrote in 1861 have lost none of their significance: "I would therefore beg the readers wherever doctrines, pages, and even chapters, occur similar, or nearly similar, to what they may shortly before have read in books or journals, simply to compare them with the first edition of this work which appeared in 1845." Describing the states of melancholia as most frequently appearing to be the direct continuation of some painful emotion dependent upon some external influences, he notes the cases without apparent moral causes in which it "does not originate as their direct continuation, but only shows itself after these affections have wrought considerable disturbance in the functions and nutrition of the nervous system, or have undermined the entire constitution." We have learned to distinguish neurasthenic conditions from the "hypochondria" of those days; Griesinger observed that the states of painful emotion may "proceed from a strong *feeling of bodily illness.*" Going further, he regarded the "states of mania as engendered by melancholia" and as a "still deeper destruction;" he never lost sight of the principle which is fundamental in all progress in the modern treatment of insanity.

inquiry and has neglected the study of its most pregnant problem, leaving it to the physiologists and the chemists to be solved for us.

The Chemical Laboratory

The chemical laboratory was the one first conceived to be a clinical need. Psychiatry was not yet ready for biochemistry 25 years ago, nor for explanations in disordered metabolism of morbid mental moods and activities; neither was organic chemistry yet able to offer practical aid to the mental clinic. But the principle being established and the way opened for its methods, the later history of these laboratories shows the somewhat indirect and obstructed path by which psychiatry is now coming to know its need. In the last 20 years there has been wrought a great transformation, culminating in the new conception of the psychopathic hospital. Dr. Folin gave new character to the expert application of pure chemistry to the problems of nutrition in psychiatry; his research work continued from 1900 to 1909, when, having an appointment in the department of biological chemistry in Harvard Medical School, he was succeeded by Mr. Erdmann; in connection with clinical pathology there was a steady advancement in the recognition and formulation of new and definite problems for chemical investigation. Established since 1900, there are now at least 14 other psycho-pathological laboratories in connection with institutions for the insane and defective in this country; in a few of them research in biochemistry is also included in their investigations, notably at Vineland by Dr. Goddard. It could not be learned at the time of the founding of its own chemical laboratory at the McLean Hospital that there was any other one of the kind in existence, in like institutions, aiming at a permanent association with the clinical work. The exception should be noted of the chemical laboratory of the new asylum at Claybury (1896), where the idea of Mott and Halliburton appears to have been to find what chemical changes took place in definite diseased tissues. Individual researches in such subjects were published prior to 1900, and under the influence of the later widely extended interest in pathological chemistry; but comparatively recent have been the efforts to discover metabolic abnormalities in the insane as a special problem by organized methods for prolonged investigation. It should be mentioned that in 1910 a laboratory of this kind was opened at the Munich Psychiatric Clinic, where the tendency of research is toward the study of changes in definite forms of organic disease. It can be said at least that the chemical laboratory at the McLean Hospital was an early attempt to carry out the "hospital idea."

The Psychological Laboratory

The psychological laboratory, under the conditions which grew up around it, as we are now prepared to see, pursued a productive course with well-known results. We are brought here to a point of critical interest in the great change in psychiatry in America—a movement still increasing in volume, though discarding much that is found wanting. The McLean Hospital laboratory typically illustrates some important factors in the general change. The psychological department, while pursuing its own special line of work in applying physiological experiment in abnormal psychology, had its course and development peculiarly subjected to collateral influences. To prove this one needs only to look through the brief summaries of work done, in the hospital reports, especially that of 1901 and the following years. The first of these concurrent and more or less controlling influences was in the clinical field, through bringing to this hospital the teachings of the Heidelberg school in 1897 as then developed, when Doctor Hoch was sent there for that purpose on his second mission. We know how these teachings spread and dominated psychiatric thinking in America. The greatest significance belongs to the fact that, on the one hand, the methods of physiological experiment had been brought years before through Stanley Hall direct from Leipsic, and his extended course of study in the laboratory of Ludwig, and moreover had been established here, coupled with concepts of biological chemistry; and that, on the other hand, certain methods of experimental psychology designed for the study of motor and intellectual function by measurement of time factors applied to psychiatry, came here later, also from Leipsic, by way of Heidelberg, but making no use of biochemical and little of physiological explanation. A great contribution to descriptive psychiatry was made by the doctrines of the Heidelberg school in arousing interest in descriptions of mental states. But they perpetuated with some changes of terms the long-accepted formulæ ascribed to Griesinger, except that Kraeplein passed wholly from the emotional criterion of Griesinger to that of activity by "increase or decrease" and "oppositeness," represented by "retardation" and "excitement." This appears to have led largely to the limiting of experimentation to reaction-time and motor effects, and to the insistence upon the analysis and classification of behavior thus differentiated into set clinical pictures and disease forms.

For the good of psychiatry, there should be noted in this connection the extraordinary fact that the extremes of divergence from the real teachings of Griesinger have grown out

of the immediate misconception of them when they were first published, and against which he made formal protest; but this was disregarded to the effect of making him responsible for an interpretation of his conceptions that he never meant, but that has ever since dominated psychiatry.[6] Believing that these states of melancholia and mania represented simply successive degrees of disorder of the mental and cerebral processes to the "deeper destruction" of their functional integrity, he used the term "depression" clearly in the qualitative sense of mental pain, not confusing it with the notion of something less than normal in contrast with something in "exaltation" more than normal in a quantitative sense. To the soundness of his real conceptions, contemporary with the beginning of the last half-century, is due their vitality and lasting influence.[7] These should not be forgotten when we recognize the fact that the greatest of the factors of progress formulated in the energy concept, with a steadily moving force toward culminating results, has been determining the long-

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[6] Griesinger, Mental Diseases, second edition, 1861: "In employing the term 'states of mental depression,' we do not wish to be understood as implying that the nature of these states or conditions consists in inaction and weakness, or in the *suppression* of the mental or cerebral phenomena which accompany them. We have much more cause to assume that very violent *states of irritation* of the brain and excitation in the mental processes are here very often the cause; but *the general result* of these (mental and cerebral) processes is *depression* or a *painful state of mind.* It is sufficient to recall the analogy to physical pain; and to those who imagine that they make things better by substituting *'cerebral torpor'* and *'cerebral irritation'* for 'depression' and 'exaltation' it may fairly enough be objected that the melancholia there is also a state of irritation."

[7] Richard Mead, Medical Precepts and Cautions, 1755: "Medical writers distinguish two kinds of madness . . . but with this difference, that the one is attended with audaciousness and fury, the other with sadness and fear; and that they call mania, this melancholy. But they generally differ in degree . . . sometimes take each other's place and undergo various degrees of combination." Among the leaders in modern British psychiatry Clouston wrote 30 years ago of the "Descent to Dementia" through melancholia and mania. Twenty years ago Bevan Lewis wrote of melancholia and mania: "Yet, fundamentally different as these mental states would appear to be, we have little doubt that the process of reduction is the same for both, but in maniacal states the dissolution is to a *greater depth*—the difference is one of *degree.*" . . . "In mania we must recognize that the excitement of lower levels is one of disorderly, ungoverned license, indicative of the removal of the influence of higher controlling planes." Here are applied in psychiatry the physiological conceptions of Hughlings-Jackson; they are in harmony with Wundt's theory of functional capacity, and the teachings of Sherrington, which are having a notable influence upon modern thought.

impending revolution now going on in psychiatry. Its tendency to this in recent years has been simply a pervasive coordinating movement toward the principles established in general medicine, to which Barker refers with respect to the value of a functional conception of pathology; medicine becoming more scientific, classifications of "clinical types" are replaced by those of "a developmental or genetic character."[8] True to the genetic method are the new investigations and discoveries in experimental physiology and biological chemistry, proving the remarkable influences of the interdependence of mental and emotional states and physiological adaptations. The new knowledge of such normal reactions is of the utmost importance for psychology and psychiatry; interferences with normal mental reactions in the functional pathology of behavior demand the tracing back to the most fundamental of all the forces that act; there can be no action, however complex the "contest of forces," that is not conditioned by the degree of integrity of the potential energy. The new psychiatry must be founded upon such explaining elementary principles. Anatomical pathology gives us end results.

Proof of the foregoing statements appears in the operation of the concurrent influences that qualified the work of the laboratories here described. The second of these major influences in relation to the course of development of the psychological department was its immediate association with the biochemical research. This is a part of what is shown by the briefest outline of the later stage of progress and by the titles of papers published. The psychological laboratory was newly organized upon a special fund in 1904; under the direction of Dr. Franz, then appointed; it demonstrated the value of such research in pathological psychology through his qualifications as a trained psychologist and his experience in the teaching of nervous physiology. Continuing till 1908, he was then appointed to establish a like service at the Government Hospital in Washington. His successor in the former service, Dr. Wells, has maintained its continuity and carried forward its development by his well-known original investigations. The record of the work done in the three laboratories indicates their stimulating influence as intimate adjuncts of the clinical service. Certain subjects of research show not only the local

[8] Barker, L. F.: Methods in Medicine, *Boston Med. and Surg. Jour.*, June, 1905: "As medicine has become more scientific, the mind has ceased to be satisfied with such descriptive classifications as the clinical symptoms and syndromes represent and with 'clinical types' set up, and is ever on the alert to replace them by classifications of a developmental or genetic character."

trend, but the general movement in psychiatry toward the conceptions of general medicine.

While psychiatry has been seeing the remarkable extension of interest from a rigid morphologic neurology on one side, to the extremes of speculative psycho-analysis on the other, it keeps to its course on the middle ground of the graver insanities where psychology and psycho-pathology are held to the stern facts of associated physical disorders. The laboratory movement, for the bringing together of the long-disjoined paths of progress of psychology and psychiatry, has a significant example in the present work of the combined clinic and three laboratories at the McLean Hospital, an environment in which academic psychology has had some years of continuous collaboration.

Some particulars of this combined service should be cited here to explain the conclusion to which this review is leading. "A constant attempt has been made to find and apply such psychological and other scientific methods as can be made practical." "A stage of transition in the general laboratory policy was reached when the former conventional methods of experimental psychology proved to be of limited usefulness; the earlier methods of the experimental study of motor and intellectual function by measurement of time factors tended to be outgrown." "In accord with the tendency of the time, a wider use of physico-chemical methods in biological research" was adopted; "attention was given to serological investigations," studies of "psycho-galvanic phenomena in relation to emotional reactions" were published. "The principles involved in the biological point of view in psychiatry" were applied; "new means were sought for experimental observation," the laboratories were extended and refitted to meet the new problems. In the later chemical work a method for the determination of the "surface tension of liquids for biological purposes" was published, and a "research on alkylamines" was concluded. "Experimental studies in association opened new fields of research, one of these being concerned with the use of a method of this nature in different forms and stages of the psychoses, and the other with "the traits of personality which it reflects among individuals in general." "In the development of a method for the systematic observation of the personality, susceptible to quantitative treatment, emphasis was laid upon the actual mental difficulties to which the individual is subjected and their proper means of adjustment." "Recognizing the most hopeful tendency in psycho-pathology and normal psychology as founded on the conception of the mind as an adaptive mechanism, an experimental method is needed for the estimation of the adaptive reactions."

The process of applying the methods of normal psychology to the problems of abnormal conditions, and of testing the validity of current contributions to psychology, has had a free and liberal field and competent direction. "The clinical ideal of the study of the whole life and personality of each patient as an individual special problem has become also the psychological ideal." This implied "the need of studies in dynamic psychology, and the investigation of the relation of mental states to the disorders of digestion and nutrition." Time

was spent at the Carnegie Nutrition Laboratory in Boston. The immediately available expert aid of "physico-chemical methods in biological research" had its influence and "determined their wider use here in accordance with the now recognized tendency of the time." (From annual reports of the McLean Hospital.)

The productive results of the combined operation of these laboratories are traceable directly to the projects of the decade 1880-90; they justify the forecast of the essential value of the dynamic concept in psychiatry, although the recognition of it has needed the labor of many years to make its wider use the tendency of the time.

The outcome here is typical of the great interest that has arisen in a considerable number of the institutions throughout the country and of their increasing productiveness within a few years. The laboratories of the McLean Hospital and the U. S. Government Hospital have tended to specialize for the longer time in psychological studies, the former giving more attention than any others to biochemical investigations. A special significance also attaches to the events at the McLean Hospital because of the introduction there of the variations in the formulation of psychiatric doctrine brought from the Heidelberg school, and because of the results of the contact there of the still prevailing system of essentially descriptive psychiatry, with the movement for broader biological explanations. The tendency of the former has been to perpetuate the fallacies of metaphorical descriptions of behavior, and to continue to seek to differentiate new "disease forms" under new names while now becoming constrained to place its failures in a growing group of "unclassified forms"; the tendency of the latter is to the practical conclusion that a functional conception of mental diseases leads to treatment through the study of the whole personality of each individual case. "Psychiatry belongs to general medicine, and mental disease, like bodily disease, is not an entity nor an agency, but the result of normal forces acting under abnormal conditions; the problem requires the investigation of the developmental and genetic character of functional modifications."[9] Osler asserts: that "the battle ground of medicine in the near future will lie in the fields of clinical chemistry and metabolism."

Conclusion

The foregoing recital in outline of events in the progress of psychiatry has needed some detail to show their historical import. While the account in part relates largely to one line of

[9] Op. cit. "The Problem of Psychiatry in the Functional Psychoses," 1905.

advancement and to one institution, for the sake of its continuity and coherence, it is still only typical of the great movements and progress of the time. It is shown that in the march of development certain forces, alike in general medicine and psychiatry, and not less in psychology, have held their course, though with unequal steps. The historical meaning, revealed by a brief tracing of these trends to their recent leadings, betokens the momentous change now going on in the conceptions of mental diseases.

The problem of overcoming the barrier between mental physiology and mental pathology is one of the greatest importance to psychiatry. That medical training in psychology is desirable needs no saying. It is needed to consider whatever there may be in the methods of normal psychology that does not fit with the problems of pathological psychology.[10] What is there in the mental attitude of the psychologist which differs from that of the research worker in the physiological or physico-chemical laboratory, who has to deal with the physical facts of the mechanism of life?

The history of the laboratories here described reveals such a difference of attitude, and shows that both the psychologist and psychiatrist have been halting between the two leadings—the latter having to compound this disharmony in his practical work. In the course of these two trends of progress in modern psychiatry, there were conflicts and mergings, and the tendency to the emergence of clearer conceptions of scientific psychiatry. In the contemporary movements on the normal plane, beginning about 75 years ago, Johannes Müller, the founder of modern physiology, and his followers developed the methods of experimental research contributing to the rapid advancement in general medicine. Wundt, who had been with Helmholtz at Heidelberg, went to Leipsic in 1872, where Ludwig's laboratory became a center of interest for American physiologists. Academic psychology was seeking in the physical field explanations to support its views of psychic activity. Wundt established his laboratory in 1879 for applying the new mode of the exact methods of physical science to psychology.

The point of present interest in this movement is in the psychology of the emotions and the accepted fixed conceptions of their associated physical contrasts. Wundt's theory

[10] F. L. Wells, "The Advancement of Psychological Medicine," the *Pop. Sci. Monthly,* Feb., 1913. "The discourse of the medical man is one of problems, of the psychologist, one of methods; which under present conditions could scarcely be otherwise. The difficulty is that the methods of normal psychology and the problems of pathological psychology do not fit."

of the "three dimensions of feeling" expressed in pairs of "opposites,"

agreeableness	excitement	strain
disagreeableness	repose	relaxation

became a large problem of psycho-physical research. This great experiment and the vast literature written around it in 30 years in volumes of description and discussion, to fit with conscious experiences a like oppositeness of normal organic reactions, has been most productive in broadening the fields of research, although a negative conclusion has emerged concerning the "three-dimension theory."[11] The psychologists have done their part along their lines of approach to the recognition of the problem of the mind as an adaptive mechanism. But to the same end, and proceeding from the rapid advancement in physiological and physico-chemical experiment, a revolution of ideas has been wrought, of which an example is the final dislodgment of the ancient conception of an oppositeness of physical reactions of "integration and disintegration," through the later discoveries proving the protective relations between normal emotional and physiological reactions. In the normal field this strengthens the foundations for cooperation in the merging of the problems and methods of psychology and medical research.

In the abnormal field the special place of these laboratories may now be pointed out by briefly recapitulating the meaning of some of the main events here narrated. In conclusion it remains also, with respect to the notable influence of the Heidelberg school, to specify more particularly what its doctrines were with reference to the results of their contact with the purposes of the combined laboratories; the manner of their introduction has been described. The beginnings of modern psychiatry are ascribed to Griesinger, who, in the awakening of his time, recognized the deeper physiological truth which he failed to impart. In the order of nature and universal experience the contrasts of mental pleasure and pain have been associated with many of the implications of exaltation and depression. The alienists rested upon Griesinger's verbal formulations; these have served the purpose of describing "clinical types set up," and even tended to antagonize the

[11] A Study of the Relations between Certain Organic Processes and Consciousness. J. R. Angell and H. B. Thompson. *Psych. Rev.*, Vol. 6, 1899.

Organic Changes and Feelings. J. F. Shepard. *Am. Jour. Psych.*, Vol. 17, 1906.

Elements of Physiological Psychology, Chap. VII. Ladd and Woodworth. New Ed. 1911.

seeking for explanation through physiological principles with which, in fact, they do not fit.

The psychiatric movement has been substantially governed by the practical principles of the "supporting treatment"—the energy concept, under the influences of general medicine, which prompted the founding of the McLean laboratories, in their combination, and the change proposed in 1887 of the Griesinger formula to fit both the mental and physical facts. The inspiration to psycho-physical research then brought by Stanley Hall from the laboratories of Ludwig and Wundt, chiefly the former, did not change the physiological attitude; it was sought to escape from the domination of the ancient descriptive conceptions lacking explanations of the obvious changes of physical functions. When later the teachings of the Heidelberg school were brought in, they proved to revive the doubted formula; with some words of elaboration upon the model of the "three-dimension theory" the "triad of opposites" was framed and came into general use:

exaltation	excitement	flight of ideas
depression	retardation	difficulty of thinking

with differentials of "increase and decrease" and determinations of disease-forms by reaction-time experiments.

This position was essentially descriptive and exactly contrary to that of Griesinger and the original purpose of the laboratories. It had no leaning toward the proposed physiological and physico-chemical explanations; the four missions of inquiry to psychiatric centers in Europe, 1888-1901, found no prolonged measures established for such combined research, but rather a lack of hopeful views of it. The clinical field of the McLean laboratories became the scene of an attempt to harmonize the perpetuated descriptive attitude and the explanatory attitude. In the first years the former flourished more, but the latter persisted. In the later period of nearly 15 years of expert work in "pure chemistry" and "pure psychology" conjoined in the clinic there emerged another change of scene indicated by the quoted description of some of the work done. The earlier conventional methods of measurements by time factors were proved to be of limited usefulness. There was increasing recourse to physico-chemical investigations of nutrition and other problems, and to new collaboration in methods for "studying the problems of the whole man" immediately presented by the physical facts of the clinic.

In the greater experiment formed by the years of work of these laboratories the primary problem was held in the proposition that all the activities represented by behavior, whether

normal or abnormal, are always conditioned by the state of the energy potential, whether adequate or inadequate, modified or inhibited by interferences. The field of mental disorders is nature's laboratory, where the psychologist's methods of analysis must fit both the mental and physical problems of the psychiatrist. The energy concept being held as implying the storage of energy in living substance, and the law of physiological use as implying growth in functional power, these physical facts are extended by the new advances in the physiology of protective and defensive reactions. This new proof sustains for psychiatry the conception of the effects of overuse, waste in excess of repair, irritable weakness with lowering of thresholds, failing inhibition with increasing activity tending to losses through exhaustion by degrees of sensori-motor function to states of lethargy, and death. In the growing recognition of the constant presence of such elementary principles conceptions of a developmental or genetic character emerge for dealing with the complexes of abnormal mental and physical conditions. Thus there is revealed the broader field of explanation for the new psychiatry.

The dynamic principle long recognized in practice has prevailed in the work of these laboratories, although usage still clings to the old formulae, which do not fit the physical facts. Psychiatry, by laying broader foundations, is becoming more completely free to frame its creed with a new ritual upon the coming revelations of physiology.

Note.—In this account of the laboratories only brief reference could be made to the genesis and course of forces proceeding from psychology into psychiatry and their relations to the modern advancement in the medical sciences. Yet the leading purpose here is to indicate the causes and import of the general laboratory movement in the development of psychiatry; this being still in its early stages in the seeking for clearer theory to fit the facts underlying the great progress in psychiatric practice and to fix attention upon the chief interest, the account of the contributive work of these laboratories is kept, as nearly as possible, objective and impersonal. But the outcome of all prevision in such matters being dependent upon the responses of the individual workers, it is of interest to psychology to note how those brought into contact here with the interacting influences of the combined laboratories and clinic produced substantial proof of the soundness of the method for the mutual promotion of progress in all these special fields.

It is due to the laboratory workers to make further acknowledgment of their efficient zeal and their valued contributions responsive to the opportunity. These being aids to the clinical service, this remains the substantial and lasting basis of the whole work. A true estimate of this can not be made without a large measure of recognition of the essential value of the intelligent co-operation of those engaged in the clinical service. Acknowledgment of his im-

portant work in this service is due especially to Dr. Tuttle through his long service since 1879, and to Dr. Abbot since 1890 except a brief interval, to the present time. It ought not to be omitted here to note the salient facts of the progress in treatment and its concurrent influences in clarifying the related psychologic and psychiatric problems. From the beginning of this period of nearly forty years there was a continuance of the tendency to the lessening use of controlling drugs as adjuvants to the "supporting treatment" in which the alienists had been the leaders of the century. A summary of the marked progress in this regard was published in 1894.[12]. Dr. Tuttle's continued contributions to this progress, through conservative, painstaking observation and disposition to prove all things in theory and practice, made so much practical advancement in the use of physiological therapeutics, both in the physical and mental fields, that he was able to make the following report in 1913: "No striking changes in methods of treatment. Emphasis is still laid on the superior advantages of out-of-door exercise, full feeding, and hydrotherapy for its tonic or soothing effects as against sedative and hypnotic drugs, which practically are never prescribed."

The methods of treatment described are an achievement as original and independent as any in practical therapeutics by the tests of experience,—a logical demonstration of cause and effect. They have an important bearing upon the validity of the psychological observations in the combined services as essential aids in the shaping of psychiatric progress by the exclusion of artificial interferences with the study of both psychopathological and associated normal adaptive reactions. Case histories in the literature concerning "disease-forms" are always open to question when the "medication" is not mentioned; the common omission of this detail of treatment suggests the possible and often probable sophistication of symptoms by drug-effects. It is significant of the coordinating influences in the combination of these services, toward the long desired overcoming of the barrier between mental physiology and mental pathology, that Dr. Abbot's "adaptive reaction" has been from his early work of a physician in the wards to the direction of the special clinical and pathological departments and to the attitude of a psychologist whose contributions are informed with thorough comprehension through a practical psychiatric training.

The interest to psychology of such an account as this is a very special one in the universal movement on all sides for the solving of the chiefest of all problems. To the master science the contributive work on the physical side is being mostly devoted to the study of the causative effects of organic diseases upon mental integrity; on the other hand there are extremes of the admirable zeal in the analysis of the thinking process tending to exclude attention to the physical facts, and promote the continued formal study of "clinical types set up,"—the progressive spirit working along lines bounded by traditional conceptions. The interest of the present situation to psychology is that when it turned to physical facts, physiology and psychophysics, it laid the sound foundation upon which the genetic method is building up impregnable constructive conceptions, this process being concurrent with the trend of the same forces in general medicine. After twenty years or more psychology had freed itself by experiment from the bondage of the ancient conceptions embodied in the "three di-

[12] Progress in the Care and Treatment of the Insane during the Half-Century. E. Cowles, *Am. Jour. Insanity,* 1894.

mension theory," as Angell and his students showed the conclusion to be. Yet psychiatry has continued, for almost another score of years, to "set up" the motive factor of feeling, which lies at the basis of adaptive reactions, in terms describing the true contrasts or "oppositeness" of the *qualities* of mental pleasure and pain but in words of double meanings falsely implying a like "oppositeness" of the physical facts. Griesinger and others knew better through the more fundamental energy concept, but that availed nothing; medical literature has come to be full of reliance upon it; but psychiatry, still unable to work out explanations through its misconceptions, turns away from the wealth of newly known facts in the physiological mechanism saying "the symptoms are all we have." Can the question be answered: Why continues this disjunction,—this resistance since Griesinger's time? Is it something inherent, atavistic? Our prehistoric forebears found comfort and safety in the trees,—they feared the prowling dangers of the earth beneath; they were cave dwellers in the cliffs, worshipped the sun in the high heavens and feared the dragons of the great abyss in the waters under the earth. Have we this disposition ingrained, solid—something that must stay, ever to be interred with our bones? The answer of psychology is that, under general principles that apply to a large class of the emotions, the motor reactions called forth as a part of the bodily resonance are adapted for the defense and preservation of the individual, and that the same or other reactions operate to the same end for the species; therefore evolutionary biology is justified in considering the bodily expressions of emotion as instinctive actions reminiscent of ancestral ways of life. (Ladd and Woodworth.)

Our inherited attitude toward life in its normal relations is set upon the instinctive conviction that we should seek pleasure and avoid pain, like ease and dislike labor, and recognize the protection of rest and the warnings of fatigue. Yet we subject ourselves to toil and pain for gaining future ease and the highest good, use our fears for defense and grow strong in the struggle for existence; we rejoice in overcoming. The psychologist, finding in this no confirmation of his former theory concerning the physical facts, may have little difficulty in adjusting his thinking to the play of the emotions on the normal plane where emotional feeling is secondary to the results of ideation and experience; he may content himself with introspection. But the physician finds his work on the pathological plane where the prevailing affective tone, both of morbid sadness and joy, proceeds from sources of real physical disorder and losses of the integrity of function. On the normal plane the emotional changes may be described intelligibly, though figuratively, as from low spirits to high spirits; both being normal they may vary alike with increase of intensity, activity, and force still under normal control. But the physician has to deal with pathological changes which always imply failure of functional integrity; the losses of control, which is the essential index of the energy content, are represented by the "irritable weakness" shown in the increase of activity in restlessness, sense of inadequacy, and consequent "mental pain;" then failure may continue through further declension of functional integrity shown in deeper sensory losses by the *fictitious* sense of well-being, with lessening control and more and weaker activity to complete loss.

It should be said that these observations, repeated in other words from foregoing pages, describe a type of cases that show such courses of reduction under stress as in Nature's experiments with normal

subjects,—such as hardy shipwrecked sailors and lost wanderers in the desert who decline through stages of despair, delirium, and exhaustion to dissolution. In the mass of cases there may be many individual variations as to which of the complex of functional factors goes first to annulment,—when there may be seeming or real stupor,—a partial or general retardation in some reflex reactions; but there is a remarkable uniformity in the essential elements of the clinical pictures, whether from regular reductions of the sustaining force or through interferences. Every psychiatrist knows the meaning of the "deeper levels of dissolution," and is governed by the principle in his practice. Here comes again the question of the resistance of psychiatry since Griesinger's time to the fundamental principle now long proven by experiment in psychology and physiology. Is it that the psychiatrist, who does not experiment, is so set in his ancestral, arborescent, cliff-dwelling, instinctive conceptions that he cannot reconstruct his ambiguous formulae to fit the facts he knows? Psychology and physiology have much to learn from psychiatry of the variations of physical facts that must condition mental activity; there should be somewhere a foundation upon which the present divergencies of psychology can come together; in its turn not the least of its duties is to persuade psychiatry to believe the meanings of the things it already knows; but much more is needed to fill the larger place that is waiting for true guidance of the master science in the preventive and curative work of the mental clinic.

Discontent with things as they are avails but little; this account is purposely limited to what has been done so far in the prolonged attempt to break the bondage of tradition and words. The first need in the present time of great promise is to set a true foundation upon which all research and psychoanalysis may build. The energy concept is most fundamental and must be reckoned with throughout; some constructive proposition is in order here,—to point the moral if not to adorn the tale. Under this conceptions the relations are shown of the essential elements of the melancholia-mania symptom groups; these are comprehended in four stages, or phases: (1) Neurasthenic, (2) Melancholic, (3) Manic, (4) Exhaustion, ending in dissolution. As there are no sharp "border lines" anywhere between the phases the expedient of "mixed phase" description must be applied alike to all of the variables of transition from one characteristic phase to another. In the typical course of lessening functional capacity the order of the condition changes can be best explained in terms of control. The losses of poise at the sensory and motor thresholds, in both the thinking and physical reactions, represent the reductions of physiological inhibition which is the index that varies directly with the energy content which may continue regularly to exhaustion and dissolution. This curve of inhibitory control consistently harmonizes, to the end, with the energy efficiency,—allowing for the variable interferences above explained. The activity, or "excitement" criterion is a false index; it varies inversely,—the more restlessness and excitement the more "irritable weakness." This metaphorically carries the graphic curves of "increase" to high and feeble levels on the *quantity* scale; thence they must drop through the whole scale of successively waning and exhausted areas of contributive action,—("the disease crosses the normal line" according to an eminent writer), in order to complete the representation of the terminal energy subsidence in retardation, stupor and death.

It should be said further that it is not meant in this account of the laboratory movement to minimize the great contributions of the Heidelberg studies to psychiatry. While the measurement of time factors appears to be of limited usefulness, with respect to the "three dimension theory" as in experimental psychology, yet the methods of case study and clinical analysis have been developed upon the true basis of the general principle of reflex action in the physiological mechanism. It is proved that these physical factors are not to be ignored, but that they furnish a guide to an orderly method for the introspection which helps to reveal not only the relations of the mental activities among themselves but also the controlling forces of the conditioning and always changing adaptations of the physiological reactions.

The conclusion that emerges may be restated here. Wundt, representative of the psychologists' attitude, formulated current conceptions in the proposition concerning the feeling-factors and sought proof for it in physiology, reaching the verdict not proven, but leading to great advancement through the precise study of physical facts. On the other hand the formulation of the energy concept was contemporary—even prior, in the practice of psychiatry. The inception of these laboratories started with this genetic principle, discarding early the physical import of the depression-exaltation formula. The validity of the energy concept, and the genetic principle as in general medicine, has been worked out in thirty years or more in these laboratories. Meantime the Wundtian theory reached a negative conclusion nearly twenty years ago. The moral of the whole matter has many specifications; among them it appears that psychology, on its part, having freed itself from the ancestral theory, has yet to apply the new knowledge of the real physical reactions that condition the expression, through the common path, of all the findings of the diverging specialists in psychology; they have yet to fit the facts that the psychiatrist knows. Psychiatry, on the other hand, finds in the pathological plane something more in the ancestral theory than its comparatively academic and negligible import for normal psychology; it has not adopted the correction that psychology and physiology have found. Having not yet freed itself from the bondage of metaphorical description, it still contents itself in its use of "forms of words" that have no physiological meaning, and block the interest in explanation. The growth in facile description and the practice of keen analysis of the mental activities (symptoms and behavior) are so satisfying that psychiatry stays superficially content with a verbal formula false in theory and fact. The force of this lingering error is surely though slowly waning before the soundness of the empirical methods and practice long wrought out. To hold the place that belongs to it psychiatry has its greatest opportunity in no longer neglecting the aid that all contributing sciences are striving to offer.

ON THE NUMBER OF ARTICLES OF PSYCHOLOGICAL INTEREST PUBLISHED IN THE DIFFERENT LANGUAGES

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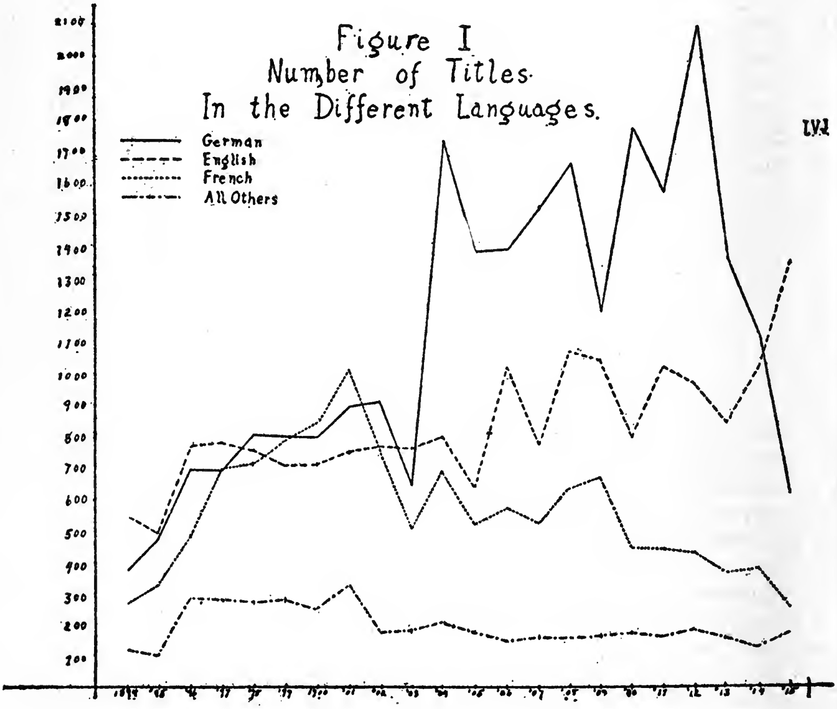
The *Psychological Index,* which has appeared annually for the past twenty-two years, is a bibliographical collection of publications which have appeared in psychology and cognate fields. There are included not only titles in the field of psychology proper, both theoretical and experimental, but also papers from related fields if they contain materials of psychological interest. Such fields as philosophy, education, anthropology, ethics, psychiatry, physiology and sociology are included. This compilation of titles is by far the most complete to be found. The editors have been in close coöperation with the *Bibliographie* published by the *Zeitschrift für Psychologie und Physiologie der Sinnesorgane* for the collection of German titles, with the *Année Psychologique* for the French titles, and with certain French and English individuals. This coöperation has been effective since the third year of publication with the exception of certain and infrequent lapses.

Hence we may believe that practically all of the articles in the field of psychology, or those of at least considerable psychological interest, may be found in these compilations. And it may be assumed that the total out-put listed from any given country may be accepted as an adequate measurement of the degree of psychological interest prevailing in that country. We do not wish to imply that the relative significance of each country's contribution to psychology can be determined from a mere perusal of the *Index;* this is obviously a matter in which total number of publications may play but a small part, but it seems possible that an indication of interest in psychological topics may be furnished by an appeal to mere number of papers published.

Several difficulties at once present themselves. If one is to determine the number of titles produced by each country in every year, one must determine the place of residence of every author. The solution of our problem did not seem to warrant such a great expenditure of labor, although such a determination would obviously have been the more correct method. To make such a determination, however, each author

must have been investigated because the language of the publication or the place of publication is by no means always an indication of the place of residence of the author.

The alternative scheme of classifying the titles with regard to the language in which they were written was adopted. Several drawbacks are apparent. For example, all German, Austrian and some Swiss titles had to be included under the general caption of German. French quite as obviously included the French articles proper, as well as Belgian, Swiss, some Italian and a few Russian. Italian was, of course, relatively pure. But the great trouble with this method of classification was that English and American titles could not be differentiated. Such a differentiation was not possible without investigating the place of residence of each author because the matter is complicated by the fact that a relatively large number of papers by English authors appear in the American journals, and also many of the large publishing firms bring out their books simultaneously in both Great Britain and



America. In our enumeration we found very few titles in any language except English, German, French and Italian. Hence we included all of the others under a single caption and in this group will be found titles appearing in the Scandinavian languages, Spanish, Dutch, Russian, Hungarian, Polish, Greek and Japanese.

TABLE I

NUMBER OF TITLES IN THE DIFFERENT LANGUAGES

Date	German	English	French	Italian	All Others	Total
1894	375	550	265	82	40	1312
1895	474	489	325	80	26	1394
1896	695	770	492	182	95	2234
1897	694	785	704	221	61	2465
1898	816	759	713	209	61	2558
1899	812	714	785	223	50	2584
1900	808	718	851	209	41	2627
1901	897	744	1015	278	51	2985
1902	923	771	759	147	28	2628
1903	661	767	513	164	17	2122
1904	1735	804	692	202	12	3445
1905	1380	651	523	160	13	2727
1906	1389	1026	584	140	6	3145
1907	1523	773	541	144	14	2995
1908	1664	1069	643	126	30	3532
1909	1195	1037	673	131	31	3067
1910	1767	800	450	151	18	3186
1911	1574	1022	445	116	45	3202
1912	2102	969	435	159	27	3692
1913	1364	853	363	123	37	2740
1914	1120	1016	383	88	35	2642
1915	619	1589	254	125	47	2634
Totals, 1894-1915	24587	18676	12408	3460	785	59916

The results of our counting will be found in Table I. In the first column will be found in order the dates of the twenty-two years covering the period of publication of the *Psychological Index* (1894-1915). In the next column will be found opposite each date the number of German titles which have appeared during that year. In the successive columns will be found opposite each date the number of titles which have appeared during the year in English, in French, in Italian, and in all of the other languages. Finally, in the last column will be found the total number of titles which appeared in all languages for each year. The bottom row of this table contains the total number of titles published in each language

during the entire period of twenty-two years. Figure I represents the same results thrown into the form of curves. Along the abscissa, the different years of publication have been laid off in order; while the number of titles have been erected as ordinates. Only four curves are represented,—those for the German, English and French titles in separate curves. For the sake of simplicity, we have here grouped the Italian titles with our group of 'All Others.' Hence the fourth curve represents the combined results of the fifth and sixth columns of our tables.

An examination of these curves and of the values which they represent proves to be of considerable interest. Let us first consider the general form of each curve separately and then turn to a discussion of the relations between the different curves.

The curve for the number of German titles starts with low values (375 in 1894); but after two years there is a very marked rise (695 in 1896). This marked rise is probably due to the fact that this year marks the beginning of the coöperation between the *Index* and the *Zeitschrift für Psychologie und Physiologie der Sinnesorgane.* We believe that the German periodicals and publications were not adequately represented during the first two years of the publication of the *Index*. But now, with the coöperation with the *Zeitschrift,* we may expect an adequate representation of the German titles inasmuch as they are collected in the home country. This coeditorship of the *Zeitschrift* and the *Index* continued for six years, and during this time the curve shows a relatively rapid and fairly steady increase in the number of titles,—reaching a temporary maximum of 897 titles in 1901. During the next two years there was apparently a temporary lapse in the relations of the *Index* and the *Zeitschrift.* Probably due to the inadequacy of the gathering of the German material during these years, the number of titles drops in 1903 to 661. It seems reasonable to suppose that this is the cause inasmuch as in the next year (1904) when relations with the *Zeitschrift* are resumed, the number of titles in the German language suddenly rises to 1735. It is very probable that this maximum, which was never surpassed until 1910 and has only been surpassed twice (1910 and 1912), is a false maximum. By this we mean that there are included in this year titles which actually should have appeared in the *Index* of the previous year. Also it is possible that this year may have been one of great production at the expense of the years before and after,—that is, many books may have appeared

early in 1904 which should have appeared late in 1903 but were held up for some reason, and similarly, many books may have appeared late in 1904 which, in the ordinary course of events, should have been brought out early in 1905. We can certainly say that the German production of 1903 and 1905 was considerably less than that for 1904.

For the eight years, 1904 to 1911 inclusive, the German production remained fairly steadily close to this high maximum. The one obvious exception is the drop in 1909 to 1195 titles, for which we are unable to give any explanation. In 1912 the number of German titles of psychological interest reaches the surprising maximum of 2102,—a number greater by 350 than has ever been attained by any other language for any year. Inasmuch as the number of German titles for 1911 and 1913 are each considerably smaller than that for 1912, we believe that this exceedingly large number is again a false maximum. From 1912 on, the curve for the number of German titles takes a surprisingly marked and rapid drop. Starting in 1912 with 2102 titles published in the German language, we find 1364 in 1913, 1120 in 1914, and only 619 in 1915. The rapid fall of the last two years is undoubtedly due to the effects of the present war. During this last year (1915) the Editor of the *Index* himself expresses the doubt that he has been able to collect all of the German titles; but during 1914 the *Index* was still brought out in coöperation with the *Zeitschrift* and so the list for this year should be relatively complete. And for this year just a little more than half as many titles are listed as were recorded two years previously.

Turning now to a consideration of the curve for the number of English titles, we find that it is very different as regards general form. It will be remembered that this curve includes both English and American productions. This curve also starts with low values for the first two years and then takes a rather rapid rise. Inasmuch as all of the four curves take a rapid rise for the third year, we believe that we may consider that this is due to greater completeness in the work of the bibliographers after the first two years of publication. For a period of nine years (1896 to 1904 inclusive) the curve of the number of titles in the English language remains surprisingly constant between 700 and 800 titles. The next year (1905) the number drops to 651 items. From now on the curve has an irregular but slowly ascending course for the ten years (1905 to 1914 inclusive),—reaching a maximum, for this period, of 1069 titles in 1908. For last year (1915) the curve has taken a remarkable upward course, reaching the

surprising maximum of 1589 titles. This is over 500 more titles than has ever appeared before in English for any one year; and this total has only four times been exceeded by the German curve. This is probably *not* a false maximum because the number of English titles for 1914 is well above the average for the last few years. We believe that this marked increase is due largely to the greater out-put of psychological literature in America. We have carefully separated the American from the British titles for 1915 with the following result. Out of the total of 1589 titles appearing in the English language for that year 1063 or 66.90 per cent were of American origin, while only 526 or 33.10 per cent were of British origin.

The curve for the French titles has again a very different form from either of those which we have discussed. It will be remembered that most of the Belgian and some Swiss titles are included under this heading. This curve starts also with low values and for the first eight years it shows a perfectly steady, although by no means regular, increase. In 1894 this curve starts with a value of 265 titles and by 1901 has the surprising total of 1015 items. For the next two years (1902 and 1903) the curve shows a marked drop,—reaching a temporary minimum in the latter year of 513 titles. For the next six years (1904 to 1909 inclusive) the curve is relatively irregular but remains approximately on a level of 600 titles. The last six years (1910 to 1915 inclusive) have seen a tremendous and almost regular decrease in the number of French titles,—reaching an absolute minimum in 1915 of 254 titles, or 11 less than were recorded in the first publication of the *Index* twenty-two years before. The decrease is very marked for the last year and is undoubtedly due to the effect of the war and the utter suspension of all scientific work in Belgium and the north of France. The editor of the *Index* for 1915 speaks of the coöperation of the French correspondents, so we may believe that the list of French titles is relatively complete.

The curve of the number of titles for all languages other than German, English and French need not detain us for very long. This curve is composed largely of Italian titles as a glance at Table I will indicate, although from time to time a number of Russian and recently a number of Scandinavian articles has swelled the totals in the column with the caption 'All Others.' This curve has a course almost similar to that for the French titles except that the variations are not so marked. It starts with low values and more or less steadily

increases to a maximum of 329 items in 1901, in which total the large number of Italian titles for that year is chiefly responsible. The next year shows a considerable drop, and from then on the curve is surprisingly regular although it shows a very slight tendency to decrease. There does not seem to be any apparent effect of the war on the number of titles for the last two years.

Let us now consider the relative courses of the various curves. That for the Italian and 'other' titles is so much lower than the other three curves that we may eliminate it from our discussion. Our discussion of the relative heights of the three curves will therefore be based on a consideration only of those for the German, English and French titles. For the first ten years of the period covered by the publication of the *Index,* the curves representing the number of titles listed yearly in the three languages under consideration, are of approximately the same height and are of the same general form. The curves cross one another frequently. Five times during this period of ten years there were a greater number of English titles than of either German or French; twice English stood in second place and three times in third. During this period German stood three times in first place, six times in second and once in third. French stood twice in first place, twice in second place, and six times in third place.

For the period covered by the next ten years (1904 to 1913 inclusive) the relations are very much changed. During this period the curves draw gradually farther and farther apart. In 1904 the number of German titles had an enormous increase,—a fact which we believe, as pointed out above, is due to the closer coöperation of the *Index* with the *Zeitschrift* on the one hand, and on the other, to the fact that the total for that year reaches a false maximum. But the German curve retains its relatively high values and is only once (1909) even approximately approached by the curve for the English titles. The French curve for this period takes an exactly opposite course and decreases considerably and fairly regularly during these years. The curve for the English titles takes the middle course,—it is irregular but has a general tendency to increase, although never markedly.

For the period of the last two years (1914 and 1915), however,—the period covered by the present war,—the relations are very different. The curve for the French titles still shows a marked tendency to decrease. The curve for the German titles also shows a marked downward tendency, which for this last year may possibly be due in part to the difficulty

of obtaining the titles from abroad. The English curve, on the other hand, shows a marked upward tendency, especially marked in the last year. Hence, in 1914 the German and English curves had come very close together,—the Germans holding the supremacy, but only by a little more than 100 titles. For the last year, however, these two curves have crossed very markedly and in the 1915 number of the *Index*, the English titles held the supremacy over the German by nearly 900 items.

TABLE II

PERCENTAGE OF TITLES IN THE DIFFERENT LANGUAGES

Date	German	English	French	Italian	All Others
1894	28.58	41.92	20.20	6.25	3.05
1895	34.00	35.08	23.31	5.74	1.87
1896	31.11	34.47	22.02	8.15	4.25
1897	28.15	31.85	28.56	8.97	2.47
1898	31.90	29.67	27.87	8.17	2.39
1899	31.42	27.63	30.38	8.63	1.94
1900	30.75	27.33	32.40	7.96	1.56
1901	30.05	24.92	34.00	9.31	1.71
1902	35.12	29.34	28.88	5.59	1.07
1903	31.15	36.15	24.18	7.73	0.79
1904	50.45	23.34	20.09	5.86	0.26
1905	50.61	23.87	19.18	5.87	0.47
1906	44.17	32.62	18.57	4.45	0.18
1907	50.82	25.79	18.05	4.87	0.46
1908	47.11	30.26	18.20	3.57	0.86
1909	38.96	33.81	21.94	4.27	1.01
1910	55.46	25.11	14.12	4.74	0.57
1911	49.16	31.92	13.90	3.62	1.40
1912	56.93	26.24	11.78	4.31	0.73
1913	49.78	31.13	13.25	4.49	1.35
1914	42.39	38.46	14.50	3.33	1.33
1915	23.50	60.33	9.64	4.75	1.78
Average, 1894-1915	39.62	31.87	21.14	5.94	1.43

It is of interest, we believe, to consider the percentage of titles published each year in the different languages. These results will be found in Table II which is compiled under captions similar to those employed in our first table. The last row of this table contains the average percentages for each language of the entire period covered by the publication of the *Index*. This percentage table is of interest because the *total* number of titles listed varies from year to year and so the relative proportions are perhaps more obvious than

those obtained from the simple distribution table. Again we may disregard in our discussion all consideration of the percentages of Italian and 'All Other' titles because they never seriously affect the general relations for the other three languages. For the period of the first ten years, however (1894 to 1903 inclusive), these titles had to be reckoned with, as they were frequently more than 10 per cent of the entire number listed.

For the period of the first ten years we find that the German, English and French languages each accounts for about 30 per cent of the total listing. Then from 1904 to 1913,—the period of German supremacy,—the German titles account for nearly 50 per cent of the items. In the year 1912 the German percentage rose to 56.93. During this period of ten years the average percentage of the German titles was 49.34. During the same period the percentage of English titles remained fairly constant,—more or less in the neighborhood of 30 per cent. The percentage of French titles for this period dropped rather steadily, being on the average a little less than 17 per cent.

When one adds the percentages for the German and English titles for the last twelve years one is astounded by the supremacy of the two languages. And we believe that number of titles in any language is a rough indication of the interest in this field by the peoples writing in that language. The average combined percentages of the German and English titles for this period (1904 to 1915 inclusive) amounts to 78.44 per cent. It is difficult to realize that nearly four of every five titles of psychological interest that have appeared in the last twelve years, has been written in either the English or German languages. The combined percentage for any year during this period has never fallen below 72.77 (1909). The highest combined percentage occurred last year (1915) when 83.83 was reached, but this was very closely approximated in 1912 with a combined percentage of 83.17.

For the sake of completeness let us compute certain totals and general averages for the entire period of twenty-two years covering the publication of the *Psychological Index*. During this entire period there are listed in all languages 59,916 titles which are of psychological interest. Of these 24,587 or 39.62 per cent were written in the German language. In all some 18,676 English titles appeared, which account for 31.87 per cent of the total. French titles number 12,408 or 21.14 per cent. The number of Italian titles for this period is very much smaller than any of the other three languages men-

tioned, being 3,460 or 5.94 per cent. All of the other languages except these four only account for 785 titles or 1.43 per cent of the total number during the period of twenty-two years.

We believe that this study, crude as it may be, points definitely to the ascendency of psychological interest among the English-speaking peoples. Even though the small number of German titles for the last year be attributed to the difficulty of importing materials, still these are all that the American student can utilize at the present time. Also the absolute number of titles written in English has steadily increased during the years of the publication of the *Index,* and this increase has been very marked indeed during the last year when the number of titles for all other languages decreased very markedly. There is also a very noticeable decline of interest in matters psychological among the French-speaking peoples. This is true from an absolute standpoint and even more markedly from the relative point of view. Perhaps the most striking point brought out by this study is the extreme necessity for the student of psychology,—no matter of what nationality he may be,—to have a facile and critical reading knowledge of both German and English.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF VASSAR COLLEGE

XXXI. A Study of Freshmen

By Sophie D. White, Sybil May, and M. F. Washburn

The class of 1918 at Vassar College was in its freshman year made the subject of certain psychological investigations. The data collected with regard to its members were derived from four sources: (1) the answers to a questionary, (2) the results of tests, (3) the marks given on the academic work of the freshman year, and (4) reports of certain students as especially 'promising,' made by members of the instructing staff to the president at his request. Each of these sources is subject to errors. The questionary answers are more or less trustworthy according to the interest of the student in answering the questions, and to her skill in introspection. The test results are affected by the interest of the student in the tests, her physiological condition at the time of testing, and an individual characteristic, distinct from the ability tested, which we may call 'ability to be tested,' the capacity to meet promptly a sudden demand for the exertion of one's powers. The marks are influenced by the preparation of the student for college, her physical condition, her interest in her work, and the personal equation of the instructor. The reports to the president were affected by the personal equation of the instructor and the interpretation put upon the rather vague term 'promise.'

The questionary was sent to all members of the class within the first two weeks of the college term. It was prefaced by the following statement: "The questions should be answered only in so far as it is possible to do so with a reasonable amount of thought: answers should not be invented for the sake of saying something. A blank may be left after any question which seems unanswerable in a given case. No marks or grades of any sort will be given to the answers, and no public use will ever be made of names."

The following questions bore on individual differences in imagery: "Can you readily remember what you hear, in a lecture for instance, or do you imperatively need to see a thing in print in order to remember it?" "Can you picture geometrical figures readily in your mind?" "Do you remember music well?" "Is the pronunciation of a foreign language especially hard for you to acquire?" "Do you have difficulty in scanning Latin and other verse?"

The following questions on 'interest' were asked: "What subjects of study do you find most interesting?" "What subjects of study do you find most uninteresting?" "Are you interested in working with your hands?" "Do you like laboratory work?" "Do you greatly enjoy poetry?"

The following questions related to endowment in language facility: "Do you remember the words of a passage readily, or the thought only?" "Do words come to you readily or with especial difficulty in an oral recitation?" "Do you like to write?"

The following questions referred to differences in the readiness with which attention is adapted to a given task, and the strength of 'de-

termining tendencies': "When out walking alone, is your attention usually more occupied with your surroundings or with your thoughts?" "Is it easy for you to turn from one task to another, or do you become so absorbed in one task that you cannot readily drop it?" "Do you work best in the morning or in the evening?" "Must you be absolutely alone in order to work effectively?" "Do you habitually plan the work of a day beforehand?" "Do you habitually carry out your plans, or do you often fail to finish your work when not compelled to do so?"

These topics were chosen because they represent individual differences readily accessible to untrained introspection, and not likely to be taken as a joke by the irreverent undergraduate, as subjects bearing directly on social and affective characteristics might have been. The endeavor was also made to frame the questions so that they could really be answered.

Two hundred and eighty-six replies to the questionary were received. With regard to the interests of the students in the subjects studied before entrance to college, the following facts appeared from these replies. (1) The ratio of the number of students who mentioned a certain subject as especially interesting, to the number who mentioned the same subject as especially uninteresting was, for history, 6.6; for English, 3.5; for modern languages, 2.9; for science, 2.6; for Latin, .76; for mathematics, .65. The interests of these incoming freshman girls thus appear to be least marked for Latin and mathematics, most marked for history and English. (2) When the number who thought a given subject especially interesting is added to the number who thought it especially uninteresting, we get a measure of the strength of the affective reaction to that subject: if the number is small, the students tend to regard the subject with indifference, being neither greatly interested nor greatly bored by it. Science has to be left out of this calculation, as not all the students offered it for entrance. Latin is regarded with the greatest indifference, the total number of extreme reactions to it being only 150; history comes next, with 161, modern language next, with 172; English next, with 196, and mathematics stands at the top, with 218; a high number which as the figures under (1) show is due to the number of persons who strongly dislike it.

Correlations by the presence and absence method were calculated for the answers to a number of the other questions.

(1) A slight negative correlation (—.21) appeared between interest in mathematics and fondness for writing. The results showed that the chances are nearly even (46%) that a student in this group who dislikes to write will like mathematics; while they are only 1-3 (34%) that a student who likes to write will like mathematics. A moderate incompatibility between mathematical and literary expression is thus indicated.

(2) A slight negative correlation, obtained also between enjoyment of poetry and fondness for manual work (—.18). Of those who enjoy poetry, only 24% like manual work; of those who dislike poetry, 81% like manual work.

(3) A still smaller negative correlation (—.16) is shown between interest in science and attention to one's own thoughts rather than to the surroundings: thus of those who are not in the habit of observing their surroundings, 24% are specially interested in science, while of those who are habitually interested in their surroundings 31% are interested in science. These percentages are affected by

the fact that not all the students had had an opportunity of studying science.

(4) The highest positive correlation, one really significant, obtained between the claim to accurate verbal memory and the possession of oral fluency in recitation. It was .41. Of the students who say they readily remember the exact words of a passage, 71% say that words come readily to them in recitation: of the students who say they cannot remember the exact words of a passage, only 42% state that words come readily to them in recitation. Of the students who say words come readily, 37% claim accurate verbal memory; of those who say words come with difficulty, only 20% claim accurate verbal memory. This is a fact that points towards the existence of specialized verbal ability.

(5) A small positive correlation, .20, is shown between ability to shift attention readily and preference for the morning as a period of work. The majority of the observers said they could readily shift attention, and that they were morning workers. Of those who shift attention readily, 75% are morning workers; of those who do not, 66% are morning workers; of the morning workers, 79% have readily shifted attention; of the evening workers, 72% have readily shifted attention. These figures lend a little support to the idea that an evening worker is a person who takes a long time to adapt himself to work. It should be borne in mind that the experiments of Gates[1] indicate that the difference between morning and evening workers is one merely of habit.

The tests were the following: a test of verbal memory and memory for ideas by the use of the "Cicero" passage (Whipple's Manual of Physical and Mental Tests, volume II, page 209), read aloud to the observer; the Reading Backwards Test, with the passage about the Indians (Whipple, I, page 334), the Hard Directions Test (Woodworth and Wells); the Analogies Test (Woodworth and Wells); the Sentence Building Test, with the words "cup, fraction, money" (Whipple, II, page 261), and the reasoning test, about the man swimming in the river, used by Mrs. Woolley in her "Mental Traits of Sex." The results of this last test had to be discarded because we failed to get enough light on the method by which the various individuals solved the problem: it is easy to guess the correct answer. A source of error was involved in the testing, in that there were about thirty different testers, all advanced students of psychology. To reduce as far as possible the variations due to the individuality of the tester, all the testing was done in the laboratory, and the only instructions given to the freshmen were read to them from statements previously distated and made absolutely uniform.

(1) The numerical results of the tests were as follows:

(a) Verbal memory, average number of words, 34.1. Highest score, 81; lowest, 6.

(b) Memory for ideas, average number of ideas, 23. Highest score, 51; lowest, 6.

(c) Reading Backwards Test, average time, 410.6 seconds. Highest score, 126 seconds; lowest, 890 seconds.

(d) Analogies Test: average time for first list, 89.5 seconds; shortest time, 27 seconds; longest time, 285 seconds; average time for second list, 80 seconds; shortest time, 22 seconds, longest, 180 seconds.

[1] Gates, A. I.: Variations in Efficiency During the Day. Univ. of Cal. Publications in Psychology, Vol. 2, No. 1, 1916.

(e) Hard Directions Test: average time, 153 seconds; shortest time, 38 seconds; longest time, 550 seconds.

(f) Sentence Building Test: average number of sentences, 5.5; average number of words per sentence, 15.6.

We undertook to correlate the test results with the class records of the students, by the following method. The system of marking at Vassar recognizes three grades only above passing; A, excellent; B, good; C, passing. The number of credit hours required for the successful completion of the freshman year is 30. In our calculation, each A on a student's record in the office was counted as 3; that is, if she received A in a three-hour course we counted 9 digits to her credit. Each B was reckoned as 2, and each C as 1; D's, or failures, were counted as minus 1's. By adding these numerical credits we obtained for each student a number representing her class-room work according to the instructors' judgments: these numbers ranged from 90, for a student who obtained a grade of A in all her 30 hours, to 30, or below in the case of students who failed in certain subjects. The distribution of the values thus obtained, however, was not such that indexes of correlation between them and the test performances could be found, for as a rule there were a number of students whose class performances were represented by the same number. We therefore selected, in the case of each test, the names of the students who stood in the first quarter and the last quarter when the results for that test were arranged in order, and calculated the average class standing of these two groups. If there was a marked difference between the average class standing of the first 25% of observers in a certain test and that of the last 25% of observers, we might conclude that positive correlation existed between excellence of class-room work and the ability tested. The average class-room standing of the entire class was 51.1.

For verbal memory, the average standing of the first 25% was 63.
For verbal memory, the average standing of the last 25% was 39.
The difference was 24.

For memory for ideas, the average standing of the first 25% was 62.
For memory for ideas, the average standing of the last 25% was 44.
The difference was 18.

For the Reading Backwards Test (speed of verbal perception), the average class standing of the first 25% was 63.
The average class standing of the last 25% was 44.
The difference was 19.

These three tests furnished the highest degree of correlation with the class-room performance of the observers. For sentence building (here the observers were ranked by the product of the number of sentences written and the number of words per sentence), the average class standing of the first 25% was 53.8. The average class standing of the last 25% was 41.8. The difference was 12. For the analogies test (average of both series), the average standing of the first 25% was 56.5; that of the last 25% 45.9; the difference was 10.6.

The Hard Directions Test correlated most poorly with class standing. For the first 25%, the average standing was 56.3; for the last 25% the average standing is 49. It appears that being slow at the Hard Directions Test does not imply academic performance noticeably below the average. It will be recalled that Meumann[2] distinguishes between an executive and a scholarly type of mind, according as one is able quickly to shift one's *Aufgabe* or is held by a persistent

[2] Intelligenz und Wille, S. 21.

Aufgabe: the scholarly type of mind might be expected to be slow in the Hard Directions Test, which demands rapid shift of attention and problem.

As an index of the presence of the kind of mental ability likely to produce high marks, the results of several tests taken together are most significant. There were eleven students who fell in the last quarter in three or more tests. Of these none quite reached average class standing (51.1): the rankings were 50, 50, 43, 45, 36, 33, 27, 24, 24, 21, 18. Of seven students who were in the last quarter in two tests, none reached average class standing.

There were seven students who were in the first quarter in four tests. One of these, whom we will call T., had only an average class standing: the standings of the others were 63, 66, 75, 87, 90, 90.

There were eight students who were in the first quarter in three tests. One of these (M.), had a class standing slightly below the average, namely, 48; one (W.) was just average; the standings of the others were 60, 75, 75, 78, 81, 84.

Thus it appears that excellent performance in several tests usually goes with excellent performance throughout the work of the freshman year; while very poor performance in several tests is correlated with a performance below the average in the work of the freshman year.

We obtained, finally, some interesting data connecting the test results with the commendation of certain students as especially 'promising' by their instructors. There were 88 students thus commended. Of these not one was found in the lowest quarter of excellence in more than one test. On the other hand, of the seven students who stood in the first quarter in four tests, every one was commended for promise, including the one (T.) whose class standing was only average. Of the eight students who were in the first quarter in three tests, six were commended for promise, including one (W.) whose class standing was only average.

XXXII. Directed Recall of Pleasant and Unpleasant Experiences

By Mildred F. Baxter, Koto Yamada, and M. F. Washburn

The motive which led the senior author of this study to devise its method was the desire to explore possibilities of testing the temperamental characteristics of individuals. As might be expected from the great difficulty attending such investigations, the most definite results of the study relate not to individual psychology but to general psychology. However, we do not feel wholly discouraged with the results from the point of view of individual psychology.

Briefly, we wanted to see whether we could get anything like a test of the 'optimistic' or 'pessimistic' tendencies of individuals from the readiness with which, under definite instructions to do so, they recall pleasant and unpleasant personal experiences. The method was as follows.

The observer was first put through a practice series in ordinary free associations, the first thirty words of the Kent-Rosanoff series being used as stimulus words. The association times were taken with a stop-watch. Then the following instructions were given: "I shall now give you two other series of stimulus words. To the first

set you are to respond by rapping on the table as soon as the word has suggested an unpleasant personal experience which you have actually had in connection with the thing signified by the word. For the second series you are to respond in a similar way when the word has suggested a pleasant personal experience. If you fail to recall such experiences within fifteen seconds I shall give you the next stimulus word. I shall give you five words from the first series, then five from the second, then five from the first, and so on in alternation. Before changing from one series to another I shall give you the signal 'pleasant' or 'unpleasant.'" The reaction times were taken with a stop-watch. After each response, the question was asked, "Was the unpleasantness (or pleasantness) you thought of physical or mental?" The stimulus words used were sixty more from the Kent-Rosanoff series, thirty each for the pleasant and unpleasant recalls. In order to eliminate the possibility that one set of thirty might naturally have more pleasant or unpleasant suggestions than the other set, we used each set for the pleasant reactions with one-half the observers, and for the unpleasant reactions with the other half.

The average reaction time for the pleasant experiences in the case of a given observer was divided by her average reaction time for the unpleasant experiences. There were sixty-nine observers, young women college students. Nineteen of these gave average reaction times which were shorter for the unpleasant recalls than for the pleasant recalls. In eleven of these cases the ratio of the pleasant reaction time to the unpleasant reaction time rose above 1.2. For thirteen of the fifty observers whose average time for recalling pleasant experiences was shorter than their average time for recalling unpleasant experiences, the ratio was .75 or below. Did the former group contain the most "pessimistic" observers of our collection, and the latter group the most "optimistic?" Would a person in a gloomy mood or of a pessimistic temperament recall unpleasant experiences more quickly than a person in a cheerful mood or of an optimistic temperament? James's principle of "emotional congruity" as a determinant of association will be recalled in this connection. It might be suggested that a person in a gloomy frame of mind would have his reaction time to unpleasant recollections lengthened by the emotional disturbance they would occasion. In our experiments, single very long reaction times, such as might be regarded as complex indicators, were omitted in reckoning averages.

We selected, for each of the observers who gave a ratio of .75 or less for the average 'pleasant' reaction time divided by the average 'unpleasant' time, three persons who were well acquainted with her personal traits, and were uninformed as to the purpose of our investigation: these persons were asked, "Do you think that A. (the observer in question) tends in general to be optimistic and cheerful, or pessimistic and uncheerful?" Similar inquiries were made for each of the observers who gave a ratio of 1.2 or more. If quick recall of unpleasant ideas indicates a pessimistic temperament, then the former group were the cheerful observers, the latter group the pessimists. Twenty of the friends of the former group judged them to be temperamentally cheerful; thirteen judged them to be temperamentally uncheerful; a ratio of 1.5. Nineteen of the friends of the latter group judged them to be temperamentally cheerful; eighteen judged them to be temperamentally uncheerful; a ratio of 1.05. Thus the testimony of friends offers some confirmation of the idea that there is a positive correlation between a cheerful temperament and especially

slow recall of unpleasant ideas. Obviously the method needs further development.

More definite results appear which bear on general rather than individual psychology. In the first place, it is evident that for the majority of the observers (72%) the average time for recalling unpleasant experiences is at least slightly longer than that for recalling pleasant experiences. The difference in time is very slight, however: the average reaction time for pleasant recalls is 3.05, with a m.v. of .24; the average reaction time for unpleasant recalls is 3.35 m.v. 19. That recall of pleasant experiences occurs more readily than recall of unpleasant experiences is further indicated by the number of zero cases, or cases where the observer failed in fifteen seconds to associate any pleasant or unpleasant experience with the stimulus word. The total number of zero cases for the recall of unpleasant experiences was 144; for the recall of pleasant experiences it was only 90.

Further, a rather curious relation appeared when we reckoned the total number of cases where the pleasant or unpleasant experience recalled was reported as physical and the total number where it was reported as mental. Obviously much inexactness in the use of these terms was to be expected of our observers. The number of pleasant experiences recalled where the pleasantness was classed by the observer as mental was 733; in only 647 cases was the pleasantness classed as physical. The number of cases where the unpleasant experience recalled was classed as mentally unpleasant was only 649; the number of cases where it was classed as physically unpleasant was 734. Thus physical unpleasantness would seem to be more readily recalled than mental unpleasantness, and mental pleasantness more readily recalled than physical pleasantness. The greater readiness to recall physical rather than mental unpleasantness might be due to the fact that mental unpleasantnesses are apt to be involved with complexes and hence tend to be suppressed; quite possibly also to the fact that physical unpleasantness (usually pain) is more homogeneous than mental unpleasantness and hence easier to recall. This suggestion was made by Professor Colvin during a discussion of these results before the Columbia Psychological Club. It is hard to explain why the pleasantnesses recalled should be so much oftener mental than physical. Possibly the fact is due to a tendency to avoid recognizing that one's pleasures are physical; the habit or convention of regarding physical pleasures as unworthy and undignified.

XXXIII. Accuracy of Visual Memory and Speed of Verbal Perception in Poor Spellers

By Annette Howell, Lucile Hopson and M. F. Washburn.

The English Department of Vassar College has the custom of selecting each year a group of conspicuously bad spellers from among its students, and subjecting them to special training in spelling. It occurred to us that an opportunity was thereby offered to the Department of Psychology for an investigation of the psychological characteristics of this selected group. We have to present in the following paper the results of certain tests made upon forty-eight notably poor spellers and an equal number of good spellers. The good spellers were selected simply on their own testimony; but bad spelling seems

to be so fashionable a weakness nowadays that people are in general willing to admit the fact that they are subject to it. It seems probable that a person who declares himself a good speller really is at least fairly good, since no social opprobrium attaches to the bad speller.

Everyone who has considered the problem presented by the constitutionally poor speller has weighed the possibility that such persons may be poor visualizers. Two of our tests were accordingly directed towards finding how our groups compared in the matter of visual memory. We first gave our observers tests of visual-verbal and auditory-verbal memory. Two passages of prose approximately equally difficult were presented: one was read aloud twice, and at the end of the second reading the observer was asked to reproduce the passage as far as possible in the original words: the other was read through silently twice by the observer herself and reproduced as far as possible in the original words. Each of the passages was used with half the observers for auditory presentation, and with the other half for visual presentation. The percentage of words correctly reproduced by each observer for each of the two passages was found.

Since in learning verbal material that is visually presented the words may be translated into auditory-motor processes, it seemed advisable to add a test of visual memory for material that could not be thus translated. Accordingly we presented to our observers a series of ten cards on each of which four nonsense figures were drawn in red ink. The figures were composed of straight lines, usually eight lines to a figure.

Each card was shown to the observer for ten seconds, and at the end of this time she was required to draw as much as she could of the figures on the card. The experiment thus tested the accuracy of the visual memory after-image, which was represented by the percentage of lines correctly recalled. Little if any verbal imagery accompanied these reproductions, but reports on its presence were called for.

It occurred to us, also, that one of the conditions affecting ability to spell might be that capacity for quick and accurate visual perception of words which is measured in the "Reading Backwards Test." Accordingly all our observers were subjected to this test, using the material supplied by Stoelting.

The results of the various tests were as follows. In the test of auditory-verbal memory, the average percentage of words recalled was, for the good spellers, 50.3, with a mean variation of 11; for the bad spellers it was 46.6, m.v. 10.4. In the test of visual-verbal memory, the average for the good spellers was 39.4; m.v. 9; for the bad spellers 37.4, m.v. 10.2. In the test with visual non-verbal material (nonsense figures), the average per cent of lines correctly recalled was, for the good spellers, 40, m.v. 9.3; for the bad spellers 27.5, m.v. 8. In the reading backwards test the average time for the good spellers was 318 seconds, with a mean variation of 89; for the bad spellers it was 493.8 seconds, with a mean variation of 139.

It thus appears that the good spellers have as a group decidedly more accurate visual memory after-images of material that cannot be translated from visual into auditory-motor terms; and that they are as a group markedly superior in the speed with which words visually presented can be associated with auditory-motor processes. That these differences are not due to differences in the general ability of the two groups of observers is indicated by the fact that the groups are nearly equal in memory for verbal material, that is, material which can be learned in either visual or auditory-motor terms.

BOOK NOTES

Proceedings of the American Society for Psychical Research. Vol. IX, Part 1, 700 p.; Vol. X, Part 1, p. 701-1332; Vol. XI, 1024 p. New York, Am. Soc. for Psych. Research, 1915-1917.

These three ponderous volumes appear to be all studies of the same persin. The first volume embodies the study of W. F. Prince, Ph. D. The second volume continues his study, ending with the twelfth chapter on the records of automatic writing. In the third and largest volume (dated August, 1917) Dr. Hyslop writes two long chapters, one introductory and the other on the examination of hypotheses, detailed record, etc. Beginning with page 867 Dr. Hyslop discusses another case, the Patison case. An extremely elaborate index is appended.

An elementary laboratory course in psychology. By HERBERT SIDNEY LANGFELD and FLOYD HENRY ALLPORT. Boston, Houghton Mifflin Co. (c. 1916). 147 p.

The authors here aim to describe the experiments in such a manner that students who have had only an introductory course in psychology can perform them without further assistance, in a half course of five hours a week, with simple instruments. In general they should be performed by the entire class in one room; they must not be too hard, must present the essential features in method and important facts, and it should be possible to get clear-cut results capable of interpretation by the students, and the experiments should not be too fatiguing.

Understanding Germany. By MAX EASTMAN. New York, Mitchell Kennerley, 1916. 169 p.

Part I discusses the anti-German hate, the characterizing nations, Nietzsche, something to hate; Part II, the only way to end war, what is patriotism and what shall we do with it, the business cost, war psychology and international socialism, pacifists, two kinds of war, the uninteresting war, a news story from Europe.

The control of hunger in health and disease. By ANTON JULIUS CARLSON. Chicago, University of Chicago Press (c. 1916). 319 p.

Carlson summarizes in this volume the work on the stomach carried out at the Hull Physiological Laboratory in Chicago during the past four years, with special reference to hunger and appetite. He has tried to present his digest in the light of the entire biological and clinical literature on the subject, hoping that it may encourage more intensive work on hunger and appetite control, especially in the field of clinical mechanism. The seventeen chapters discussed here, together with the reports of his extraordinary case constitute perhaps the most important contribution to the physiology of hunger and digestion since the ancient case of St. Martin whom it somewhat resembles.

The causation and treatment of psychopathic diseases. By BORIS SIDIS. Boston, Richard G. Badger, (c. 1916). 418 p.

In its make-up this is an exasperating book. The type is none too clear, nor the paper too good, the front edge is ragged and uncut, and in our attempts to cut it the paper tears.

Sidis is possessed with the idea that functional neuroses are not congenital but results of defective education in early child life, a view which is natural, if not necessary, for everyone engaged in therapy. It is the attitude also of hope that appeals to patients, parents and friends. We may well hope it is true. The author appears not to make any serious attempt to add substantially to his own theories or conclusions. He still believes in hypnoidization, and his chapters on psychopathic fears and the psychophysic substratum, the impulse of self-preservation, the law of reversion, embryonic personality, the sources of psychopathies:—these are all interesting.

Mental examination of two thousand delinquent boys and young men. By M. L. Beanblossom. Jeffersonville, Indiana Reformatory, 1916. 23 p.

The author concludes that imprisonment tends to accentuate abnormalities as well as mental eccentricities. There is a relation between the kind of crime and the type of intelligence. One's vocation or the absence of it is indicative of the type of intelligence. Barring the criminal by accident or mishap, we can say in general that all have the same sort of defects, although these are often so recondite that they are hard to get at.

A manual of nervous diseases. By Irving J. Spear. Philadelphia, W. B. Saunders Co., 1916. 660 p.

The large divisions of this work are as follows: Anatomy and Physiology of the Nervous System; Examination of the Patient; Diseases of the Peripheral Nerves; Diseases of the Muscular System; Diseases of the Spinal Cord; Diseases of the Brain; Diseases of the Brain and Spinal Cord; Diseases of the Nervous System without Pathologic Findings; Neurosis Characterized by Spasmodic Muscle Contractions; Diseases Due to Perversion of Secretion of the Ductless Glands; Diseases Due to Disturbances of the Vasomotor System; Trophoneuroses; Unclassified Disorders.

Mentally deficient children; their treatment and training. By G. E. Shuttleworth and W. A. Potts. 4th ed. Philadelphia, P. Blakiston's Son and Co., 1916. 284 p.

This is an amplified fourth edition of a work published in 1895 which is too well known for detailed review. It covers the history of the subject, treats especially of defective and epileptic children, methods of special instruction, physiological classification, the forms of mental deficiency, etiology, diagnosis, prognosis, psychopathies of puberty and adolescence, medical examination, treatment, education, industrial and moral training. The merit of the book is as a compend rather than as an original contribution.

A brief history of panics and their periodical occurrence in the United States. By Clement Juglar. Third edition, translated and edited with an introduction and brought down from 1889 to date by DeCourcy W. Thom. New York, G. P. Putnam's Sons, 1916. 189 p.

Panics continue to appear as usual but are less severe since 1890. They will never cease but must be abated slowly, as medicine abates disease. This history is entirely pragmatic, giving the bald facts about each panic and attempting very little in the way of generalization.

Problems of religion; an introductory survey. By DURANT DRAKE. Boston, Houghton Mifflin Co. (c. 1916). 425 p.

This book is divided into three parts, historical, going back to the Greeks, Buddhists, Jews, devoting a chapter to Jesus Christ, the founding of the Church, early Christianity, later Christianity and Mohammedanism; 2nd, psychological, treating the God of experience, sacrifice and sin, salvation, conversion and atonement, faith and prayer, religious love and peace, the essence of religion, and the Christian religion. The last part is philosophical and treats the theological method and the scientific spirit, the interpretation of the Bible, miracles, creation and design, interpretation of religious experience, pragmatic arguments, the counter-attack on science, problem of evil, immortality, and faith.

The relations of general intelligence to certain mental and physical traits. By CYRUS D. MEAD. Teachers College, Columbia University, Contributions to Education, No. 76. New York, 1916. 117 p.

Summing up this study of children's perception and memory, the author concludes that normal children are better at each age than defectives and girls better than boys in perception, although sexes differ less with the feeble-minded; that "schoolable" mentally defective children of sixteen or eighteen are not much better in these powers than normal children of eight. Defective children are at the very lower end of a larger distribution curve for children in general; and the best mental powers defectives are likely to bring to school are perception and memory.

How to learn easily; practical hints on economical study. By GEORGE VAN NESS DEARBORN. Boston, Little, Brown and Co., 1916. 227 p.

The chapters here are, Economy in Study; Observation and the Taking of Notes; Educative Imagination; Books and Their Educative Use; Is Your "Thinker" in Order? Examination-Preparedness.

The war and humanity; a further discussion of the ethics of the world war and the attitude and duty of the United States. By JAMES M. BECK. New York, G. P. Putnam's Sons, 1916. 322 p.

This is a work of seven chapters, as follows: The "Distress of Nations"; The Submarine Controversy; The Case of Edith Cavell; The Foreign Policy of President Washington; "Where There Is No Vision"; America and the Allies; The Vision of France.

The purpose of education. By ST. GEORGE LANE FOX PITT. Cambridge, University Press, 1916. 144 p.

Fox examines educational problems in the light of recent psychological research, printing a letter-preface from Émile Boutroux. The chief topics discussed are human personality, emotion and instinct, character versus reputation, incentives to effort, economics, specialization, multiplex environment, religions, ideals and the twice-born, the production of the ideal, and lessons of the war.

Ventilation in relation to mental work. By E. L. THORNDIKE, W. A. McCALL and J. C. CHAPMAN. Teachers College, Columbia University, Contributions to Education, No. 78. New York, 1916. 83 p.

Measurements of some achievements in arithmetic. By CLIFFORD WOODY. Teachers College, Columbia University, Contributions to Education, No. 80. New York, 1916. 63 p.

Completion-test language scales. By MARION REX TRABUE. Teachers College, Columbia University, Contributions to Education, No. 77. New York, 1916. 118 p.

Adjustment of school organization to various population groups. By ROBERT ALEXANDER FYFE MCDONALD. Teachers College, Columbia University, Contributions to Education, No. 75. New York, 1916. 145 p.

Mortality statistics. 1914. Fifteenth annual report of the Department of Commerce, Bureau of the Census. Sam. L. Rogers, Director. Washington, Gov't Printing Office, 1916. 714 p.

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EMOTIONAL REACTIONS AND PSYCHOLOGICAL EXPERIMENTATION*

By John B. Watson and J. J. B. Morgan

In 1890 William James paid his respect to the then current literature on the psychology of the emotions in the following words: "But as far as scientific psychology of the emotions goes, I may have been surfeited with too much classic reading on the subject, but I should as lief read verbal descriptions of the shapes of the rocks on a New Hampshire farm as toil through them again."[1] James in his turn then gave a clever formulation of what he considered to be the fundamental principles involved in all emotion: "My theory, on the contrary, is that the bodily changes follow directly the perception of the exciting fact and that our feeling of the same changes as they occur IS the emotion."

For twenty-six years psychologists have been content with James' barren but graceful formulation. Apparently "having the goose that lays the golden egg" has so contented our experimental natures that we have not taken the time nor the trouble to encourage the goose to continue her activity. It has taken psychology many years to break away from James' fascinating way of formulating psychological principles. No better proof of this can be found than in Angell's recent contention that James' theory is still broad enough to embrace all of the recent results which have come to us from the

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[1] If anyone believes that our general treatises on emotions are any more exciting today, let him try to read McDougall's Social Psychology and Shand's work on the sentiments.

physiologist's study of the endocrine glands.[2] However satisfactory this may be for those who are not yet ready to accept a behavioristic formulation, it can hardly be denied that James' conception of the emotions leaves us with no handle for experimentation.[3] The psychologist is being constantly asked by his own students as well as by the physicians, educators, and jurists: "Why do you not work upon the emotions? They are of more importance in the guidance and control of the human organism than any of your hair-splitting work upon thresholds." Sometimes the psychologist defends himself by telling his questioners that his laboratory will undertake to work out any problems they will set him. But this is only a subterfuge. The psychologist has not attempted to formulate and spread the problems in emotions. And why should the man who needs to *use* psychological methods in a practical way be forced to do the psychologist's task for him?

Thorndike has with some measure of success broken away from the traditional formulation. In "The Original Nature of Man" he has adopted the uniform procedure in discussing both instinctive and emotional reactions of giving as best he can the original stimulating factors and the original responses. One feels though that he has done this by a kind of *tour de force*. Having waded through the enormous literature he tries to produce simplification not by adopting any central or unifying principle but by picking out several hundred of the reactions which are most universally admitted as

[2] Angell's words are as follows: "And I think he would perhaps urge that, after all controverted points are left aside, the main issues for which he would wish to contend are (1) the instinctive basis of emotional reactions, and (2) the invariable re-percussion upon the cortex of these reflex effects in the muscles, glands, and viscera. Phrase your doctrine so that these two great groups of facts are recognized and properly evaluated, and you may call your theory Jamesian or not as you please. You will at least have accepted what lies at the root of James' theory." In regard to (1) we shall try to show that while it is true it is only one of the important facts, the other one being that emotions can be torn away from the original stimuli which called them out and attached to other stimuli at will. In regard to (2) we should say that the cortical re-percussion is not essential (even if true) except to an introspective psychology. The important thing is that secretions occur which so change the physiological state of the person that he can do things which he cannot do at other times (increased endurance, strength, elimination of fatigue products, etc.). Of these important changes the person can give no account (probably *no cortical* 're-percussion' and hence no arousal of language habits).

[3] Or we should probably better phrase it, with the wrong handle. No more pitiful sight can be seen anywhere than that afforded in our libraries where the collection of plethysmographic studies upon the emotions are shown.

being present and stating their important features in an objective way. We have no fault to find with this procedure if all of the reactions which Thorndike lists belong to the original nature of man. We doubt seriously that this is the case. We have reason to think that very many of the reactions which Thorndike puts down as original in the nature of the infant are undoubtedly the product of the environment.

If Thorndike has been overly prodigal in his enumeration of the emotional reactions which belong to man's original nature, the Freudians have been too parsimonious. While they admit the presence of other emotional reactions than those belonging to the sexual sphere (using this term as Freud would himself) the latter they insist are nevertheless the ones which play the most important rôles. We may very readily admit this in most "psychogenic" cases but the doctrine is not satisfying from an experimental point of view. Such a view is entirely too narrow and it does not do justice to the other emotional reactions. Furthermore, the Freudian point of view does not help the laboratory psychologist in gaining experimental control over the whole system of emotional reactions.

We should strive to reach some formulation of the emotional reactive tendencies which will give ample scope for psychological experimentation and which will at the same time do justice to the wealth of material which modern psychiatric methods have yielded.

After observing a large number of infants, especially during the first months of life, we suggest the following group of emotional reactions as belonging to the original and fundamental nature of man: *fear, rage,* and *love,* (using love in approximately the same sense that Freud uses sex).[4] We use these terms which are current in psychology with a good deal of hesitation. The reader is asked to find nothing in them which is not fully statable in terms of situation and response. Indeed we should be willing to call them original reaction states, X, Y, and Z. They are far more easily observ-

[4] This list is identical with James' list of *coarser* emotions except for the omission of grief which James puts first. Grief we look upon as being a *reactive state* (connected with love, really) in which the object or situation which usually calls out in the subject the reactions of love is suddenly removed. The state of grief must be looked upon as a mal-adjustment period where the objects and situations which have usually called out both the original love responses and the conditioned reflexes built upon them are lacking. The state (in normal cases) disappears as soon as new objects are found or new conditioned reflexes have been entrained.

able in animals than in infants. While we do not claim that this list is complete, we do claim that our own observation of the first few months of infancy has not yielded any larger number.

Fear. What stimulus apart from all training will call out fear responses; what are these responses; and how early may they be called out? The principal situations which call out fear responses are as follows: (1) To suddenly remove from the infant all means of support, as when one drops it from the hands to be caught by an assistant. (In the experiment the child is held over a bed upon which has been placed a soft feather pillow.) (2) By loud sounds. (3) Occasionally when an infant is just falling to sleep the sudden pulling of the blanket upon which it is lying will produce the fear responses. (4) Finally, again when the child has just fallen to sleep or is just ready to awaken, a sudden push or a slight shake is an adequate stimulus. (2) and (4) above may be looked upon as belonging under (1). The responses are a sudden catching of the breath, clutching randomly with the hands (the grasping reflex invariably appearing when the child is dropped), blinking of the eye lids, puckering of the lips, then crying; in older children possibly flight and hiding (not yet observed by us as 'original' reactions). In regard to the age at which fear responses first appear we can state with some sureness that with few exceptions the above mentioned group of reactions appear at birth. These findings agree so far with the position taken by Thorndike in The Original Nature of Man. It is often stated that children are instinctively afraid in the dark (Thorndike). While we shall advance our opinion with the greatest caution, we have not so far been able to gather any evidence to this effect. When such reactions to darkness appear they are due to other causes; darkness comes to be associated with absence of customary stimulation, with noises, etc. (They should be looked upon as conditioned fear reactions.) From time immemorial children have been "scared" in the dark, either unintentionally or as a means of controlling them (this is especially true of children reared in the South).

Rage. In a similar way the question arises as to what is the original situation which brings out the activities seen in rage. Observations seem to show that the *hampering of the infant's movements* is the factor which apart from all training brings out the movements characterized as rage. If the face or head is held crying results, quickly followed by screaming. The body stiffens and fairly well coördinated slashing or strik-

ing movements of the hands and arms result; the feet and legs are drawn up and down; the breath is held until the child's face is flushed. In older children the slashing movements of the arms and legs are better coördinated and appear as kicking, slapping, biting, pushing, etc. These reactions continue until the irritating situation is removed, and sometimes do not cease then. Almost any child from birth can be thrown into a rage if its arms are held tightly to its sides: oftentimes even if the elbow joint is clamped tightly between the fingers the responses appear: at times just the placing of the head between cotton pads will produce them. Even the best natured child will show rage if its nose is held for a few seconds.

Love. The original situation which calls out the observable love responses seems to be the stroking or manipulation of some erogenous zone, tickling, shaking, gentle rocking, patting, turning upon the stomach across the attendant's knee, etc. The response varies—if the infant is crying, crying ceases, a smile may begin, attempts at gurgling, cooing, and finally, in slightly older children, the extension of the arms which we should class as the forerunner of clasping in the narrowed sex act in coitus. The smile and the laugh which Freud connects with the release of repression (we are not denying in the case of adults that this may not be true) we should thus class as original reaction tendencies intimately connected from infancy with the stimulation of the erogenous zones. By original nature the child is not *polymorpheus perverse,* nor addicted to playing with urine, fecal matter or (originally) even with the anus or sex organs. Habits (conditioned reflexes) are rapidly set up in connection with these objects at a *very* early age and they may when not looked after sadly warp the child. Jung is quite right in giving up Freud's conception of the original nature of the child, but at such an early age may habits of perversity arise that one not observing children from birth might very well suppose that the acts in question belonged to its original nature.[5] From birth the child will put a finger or a part of the hand in its mouth (native reflex connected with feeding). There is no such

[5] We should say, though, that after all Freud's conception is nearer the practical truth than Jung's present one. Where Jung can hope to get scientifically with his present excursion into the realms of folk lore we cannot possibly see—we are undoubtedly receiving at his hands interesting and really fascinating literary productions on the subject of folk lore.

original tendency to bring the hand to the sex organs. The earliest well marked responses of this kind we have seen were observed in a baby girl approximately one year old. She was in her bath playing with a small box. In her efforts to get it her hand slipped and came in contact with the vagina and entered it. A characteristic smile overspread her face and her efforts to recapture the box ceased. But at this age the child had formed varied habits of manipulation. Our contention here is that her own manipulation of the erogenous zone was brought about by the formation of a habit. That such a conditioned reflex or habit might arise at an earlier age is beyond question.[6]

It may be argued that if these three emotional reactions are the important ones and that if the stimuli which call them out are as simple and crude as we now suppose them to be, then our theory of the emotions is superficial and patently unable to care for the enormous complexity in the shading of emotional reactions in adults.

This argument brings out the chief fault we have to find with the current conception of emotion. Depending blindly upon the fact that emotional reactions are hereditary, we have put the emphasis on trying to *enumerate* the hundreds of objects and situations which produce the hundred or so of emotional reactions, instead of taking emotions as we find them and trying to put them under experimental control.

So far no one has tried explicitly to introduce the illuminating concept of habit formation into the realm of emotions. This is not quite true since the concept of *Uebertragung,*[7] however mystical and unintelligible the Freudians have made it, is nothing more nor less than habit formation (although they have not so presented it), as both Wells and Watson have pointed out. It is extremely interesting that the Freudians were the first to utilize this principle and it is more or less of a reflection upon us that we did not have it worked out ready for the use of the psychopathologist. But the analyist uses the concept only in a practical way. His experiment is made for him. The patient comes to him with the transfers already made. The psychiatrist's problem in a particular case

[6] The term conditioned reflex does not make the word habit superfluous. Habit is a series of conditioned reflexes. The conditioned reflexes are the units into which all habits may be resolved.

[7] Freud uses Uebertragung in a very narrow sense—the attachment of the patient's love-reactions to the physician making the analysis. We use the term here in the broader sense in which it is used by Ernest Jones (Papers on Psychoanalysis).

is the breaking up of the abnormal forms of attachment[8] to such an extent that the patient is placed in a position to form new and non-conflicting attachments. His interest is largely in the patient and need not be particularly scientific. The psychologist, on the other hand, cannot afford to rest until he can control his phenomena—until he can not only *produce attachments* and study the laws of their production, but also *reduce* and break them up at will and learn the principles controlling their reduction. He must find a uniform procedure which will allow at least approximate reproducibility of his results. In general, he must have his phenomena under such control that he can watch their inception, course, and end.

We have spoken thus far of transference within the sphere of love (and by *Uebertragung* the Freudian means only transference in the sphere of sexual emotion): *But there is no reason to suppose that the same thing does not occur in the other emotions.* Rage[9] likewise is capable of being attached now to one object, now to another in an ever widening series—i. e., given an original situation which will arouse rage (see p. 167) and you will have transfers wherever the conditions are at hand for the arousal of conditioned reflexes. An individual hampers my use of my arms and legs, constrains me, holds me badly when dressing me, etc. (original conditions for arousing rage)—shortly the mere sight of that individual arouses the rage components. Finally an entire stranger whose behavior is even slightly similar to that of the first individual may set off the responses.[10] [The rage that appears towards inanimate objects, stones, sticks, close confinement, etc., which hamper our movements, is probably not conditioned but is the remains of the fundamental situation and response.]

Similarly, fear being at first a simple group of responses called out by a rather definite situation (see p. 166) soon becomes attached to an ever increasing series of objects. Probably the best illustration of this is to be seen in the responses of most individuals to lightning. The hands are put

8 In view of the generally narrow use made of the concept Uebertragung in most Freudian literature it seems advisable to get some better form of expression. The English translation "transfer" has also a very restricted use. In view of this we suggest the terms "attachment" and "detachment" of the emotions, etc.

9 And we should unhesitatingly affirm that rage and fear and *not love* play the preponderating rôle in early infancy.

10 As adults we would say on seeing such a stranger: "I can't stand that person. I have an instinctive aversion to one who looks like that."

over the eyes, breathing is momentarily checked, and temporary paralysis of all movement of the limbs appears, giving place to movements which bring the subject under cover. We have never seen a child show fear reactions even to momentary flashes of light in a dark room. At most in this latter case there is a blinking of the eye lids and a slight movement of the head. Loud *noises,* however, will produce the group of reactions in question. In daily life the sudden lightning flash is usually followed by a heavy peal of thunder. The two occurring almost simultaneously afford the most suitable conditions for arousing a conditioned reflex. If the thunder is delayed five seconds or more the subject may have gone through the whole gamut of movements before the sound appears and will no longer react to it when it comes.

Recent physiological work tends to strengthen our hypothesis that fear, rage, and love are the most fundamental modes of emotional response. Crile has been attempting for some time to connect the reactions in love with glandular secretions of the thyroid but so far he has not produced very satisfactory experimental evidence, nor a very clear theoretical view. It is from Cannon's work that we get our best experimental evidence that under the influence of secretions called out from the ductless glands by the major emotions the *organism's physiological state is so changed that it can do things (and endure things) which it could not do when such secretions are absent.* This is our real evidence for the assumption that the emotions furnish the "drive" for many forms of activity. Cannon has shown that the situations or objects which call out fear and rage at the same time call out reflexly adrenal secretion and that this secretion can, by virtue of its power of exciting the sympathetic system, cause the reactions seen in those states to continue long after the exciting object has been removed. Furthermore this secretion can attack the store of glycogen in the liver and set it free as a source of food supply for the muscles. At the same time circulation is bettered and fatigue products are washed from the muscular tissue. The organism is thus put in a changed state which *may* be advantageous both for attack and defense and for the formation and functioning of habits as well. An interesting confirmation of this was recently demonstrated in our laboratory while testing the grasping reflex of infants. In many cases we find that the child either cannot at first support its full weight with either right or left hand, or if so only for an instant. If, however, we first produce a state of rage in the infant by hampering its movements we rarely

fail to get it to support its full weight for a considerable time. We here utilize in an experimental way the emotional 'drive' to bring about an increase in the strength and endurance of a non-emotional act (viz., in the grasping reflex). This leads us to our central topic. We have tried to show as best we can in the lack of more exact experimental evidence (1) that by the method of conditioned reflexes, emotional reactions can be called out by situations (stimuli) which do not at first call them out; and (2) that emotions, by virtue of the secretions which are present, furnish a 'drive' (possibility of reaction or continuance of the reaction) which is lacking in ordinary instinctive and habitual actions. These two assumptions, if they are true, should bring the subject of emotions directly into the focus of experimentation.[11] Experimentation should not be more difficult here than in any other field. The writers have already begun the following experiment: by means of a heliostat to suddenly flash a beam of light upon an infant's face, the infant lying face upward upon a table in a dark room; simultaneously with the flash a sound stimulus resembling thunder is made. Our object is to see whether the flash of light will in time come to produce the *cry* which the noise calls out.

In the same way we shall attempt to see if the bodily struggles which appear in rage may not be conditioned by a slight contact, sound, odor, or light stimulus. In animals our experimentation could be made much more thorough for there

[11] Here we should like to offer a criticism not of the Freudian psychology but of their methods of observation. They have not examined with sufficient care the concrete daily situations of infant and child life. Instead of going to the realm of folk lore for our store of illustrations, we should go to the nursery and watch the child at its bath, as it is being dressed, its early habits of bodily manipulation, its use of toys, etc. Thus instead of going to folk lore to understand much of what we see in later symbolic adjustments, we should go to the nursery not only to find the concrete explanation of symbolism but also to seek for much of the data for the understanding of folk lore

In time we hope the men behind the psychoanalytic movement will come to realize that they have not, as White maintains, built up a complete psychology differing *toto caelo* from anything which had existed before. When clearer discussion is possible we venture to predict that the one thing which will stand out as distinctly Freudian will be their utilization of the principle of *Uebertragung*. To our mind this is the essential concept in Freudian psychology. The whole group of concepts—the 'unconscious,' 'repression,' 'sublimation,' etc., will soon pass into the limbo of yesterday. Had Holt discovered the key to Freudian psychology (or as he states it, "the key to psychology") to be *Uebertragung* instead of the "wish" he would have come nearer to having pointed out their greatest contribution.

we may work directly in the sphere of the sex emotions. After such transfers or *attachments* have been brought about the process of *detachment* can be studied in a similar way. It would not be hard to plan now a comprehensive series of experiments which would have for its object the understanding of the mechanisms of attachments and detachments of the emotional reactions.

Love is a field in which early experimentation is much more difficult. On account of the peculiar situations which bring out love responses in infants (stimulation of the erogenous zones) we probably cannot safely use this emotion in its original connections. But the infant very shortly makes attachments for itself. Very soon the sight of the mother, on account of the fact that she bathes, feeds, and pets the child, begins to call out the responses of love. She becomes a stimulus substitutable for the original stimulus. Later on other persons, dolls, toys, etc., begin to condition those reactions.

If emotion can be attached in this way, and if such attachments can be made to serve a useful end such as that of helping individuals to form necessary but prosaic habits, interesting practical outcomes may be expected from our work. That our remarks may not seem wholly theoretical we wish to offer the following for consideration: students' "attachments" to teachers are very common ("crushes"). We have asked many schoolmen whether a pupil upon entering a grade and shortly forming such an attachment to the new teacher does not do better for that teacher than for the teacher in the grade below who did not call out the attachment. We have never received a satisfactory answer. Not long ago the senior writer asked this question of a somewhat distinguished body of teachers from all parts of the country. Most of them seemed to suggest by their manner that they had been asked a peculiarly personal question. The subject, however, is an objective one and needs consideration. As we look back upon our own school life we are convinced that those teachers, both men and women, for whom we had strong attachments have been the ones from whom we have received lasting benefits. Nor is the case very different with the other two emotions. A teacher who has on occasion a sharp and caustic tongue can by the use of it induce a modified rage response in his pupils which may be extremely useful in raising the level of achievement of the class. Likewise the teacher who can and at times does induce the fear reactions becomes a powerful factor in the lives of his pupils.

So convinced are we of the possibilities of getting higher incentives or drives from the use of these emotional factors that we are sure our selection of teachers would be greatly influenced by our views. We think it would be a safe move now to provide in the early grades men teachers for the girls and women teachers for the boys, these teachers to be chosen for their pleasing personalities and for their ability to attach the pupils to themselves in strong but wise friendships. This arrangement would yield the additional service of breaking the now too great fixation on the parents. We fear that one reason for the widespread homosexuality among women is due to the fact that young girls spend the greater part of their lives under women teachers. In the high school our selection again would be influenced by the extent to which the proposed teacher would call out, control, and use wisely the emotional life of the child. Hitherto our selection of teachers, when there has been any selection at all, has been made upon the basis of their erudition. Most of them are erudite enough but few of them have the gift of controlling and using the pupils' emotional life.

But the interests of the laboratory men are not more centered upon the educational field than upon the practical and vocational sides of life. In the business world the mass of workers accept a level of adjustment which is far too low. James in his Energies of Men has brought this out far better than we can hope to do. Many of our laboratory studies bring out the same factors. Unless the stimulating values are kept constant and high, stages of non-improvement or plateaux appear in the learning curves. The cry of the business man today is: "How can I control this?" The solution would appear to lie in getting the act, in which improvement has ceased, hitched up in some way with an emotional state, for in this way an added "drive" may appear and a new level of adjustment be reached.[12] Whether connection should be made through rage, fear, or love is an experimental problem.

[12] It is interesting to note here too how early this conception of the "added drive" throws light upon the fact that under the influence of high emotional stimulation outcasts from society have been known to come back and to reëstablish a stable system of habits. Oftentimes the emotional period does not last long enough for stable habits to be formed and the 'convert' backslides. Marriages to save the man when they have resulted in reformation may owe their happy culmination to this factor. Possibly a more judicious and less prodigal use of sex in the early stages of marriage (thus maintaining the high emotional state for a longer period of time) would bring about a much larger number of reformations.

Many drives have been hit upon in a practical way already, by business houses such as threatening discharge (fear), by ridicule (rage), and by getting the individual attached to the "house" through "loyalty" (love). The bonus and participating schemes have proven equally satisfactory for enhancing emotional reactions and hence in getting higher levels of adjustment.

We have developed these highly speculative considerations which have to do with educational and vocational problems to show the ready demand which there is for the scientific findings of laboratory studies of the emotions. The main contentions we have raised here concerning the possibility of detaching emotions from situations in which they are not serviceable and attaching them to situations in which they may be of service may be found to be far from correct; but if experiments are set in motion which will bring out the true relations we shall gladly retract any heresy of which we may have been guilty. We think no one can deny that the laboratory has a possible method of bringing the emotions under experimental control.

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ON THE INHERITANCE OF ACQUIRED MODIFICATIONS OF BEHAVIOR

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It has always been difficult for students of psychology to convince themselves that *some* modifications of behavior in the individual might not be inherited. This feeling that innate forms of action may require for their explanation the assumption that some of them, at least, originated through the life experiences of individuals, has been widespread, as a few typical quotations will serve to show.

"We must suppose that the physical changes which the nervous elements undergo can be transmitted from father to son. . . . The assumption of the inheritance of acquired dispositions or tendencies is inevitable, if there is to be any continuity of evolution at all. We may be in doubt as to the extent of this inheritance; we cannot question the fact itself." (Wundt, Human and Animal Psychology, Eng. tr. by Creighton and Titchener, p. 405.)

"I believe, with Wundt, that 'die zweckmässigen Reflexbewegungen stabil und mechanisch gewordene Willenshandlungen sind;' I believe, with Ward, that 'volition or something analagous to it' has, in the race as in the individual, invariably 'preceded habit;' and I believe, with Cope, that even 'the automatic involuntary movements of the heart, intestines, reproductive systems, etc., were organized in successive states of consciousness.'" (Titchener, The Psychology of Feeling and Attention, pp. 299-300.)

"Every element has shaped and tempered it [the soul]. Its long experience with light and darkness, day and night, has fashioned its rhythm indelibly. Heat and cold, the flickering of flame, smoke and ashes, especially since man learned the control of fire, have oriented it toward both thermal extremes. Cloud forms have almost created the imagination. Water and a long apprenticeship to aquatics and arboreal life have left as plain and indelible marks upon the soul as upon the body. Sky, wind, stars, storms, fetichism, flowers, animals, ancient battles, industries, occupations, and worship have polarized the soul to fear and affection, and created anger and pity." (Hall, Adolescence, Vol. 2, p. 69.)

"Although, as yet, the evidence in favor of the inheritance of acquired characters is inconclusive, it is of sufficient importance to make it impossible to disregard entirely the possibility that such inheritance has played a large rôle in adaptive evolution." (Watson, Behavior, p. 179.)

The most outspoken opponent of such views is Thorndike: "The burden of evidence is thus against the transmission of acquired mental traits. The strengthening of a connection between a situation and a response by an individual seems unlikely to modify his germs so as to reproduce, in the children developing therefrom, a stronger bond between that situation and that response than they would otherwise have possessed. Similarly for the transmission of an abolition or weakening of a connection." (The Original Nature of Man, pp. 234-235.)

It must be admitted that many advocates of the Lamarckian hypothesis in psychology have stated their positions in terms which have tended to prejudice many minds against the whole theory. They have, that is, assumed that originally the acts which were transmitted were acquired by the organism in a deliberate, or voluntary, or purposeful fashion, by methods similar to those involved in the more complicated sorts of learning in man, that they then, in the individual, became mechanised as habits, and so were transmitted. As an extreme form of such doctrines, we have the view that instinct is "lapsed intelligence;" in its more moderate expressions, the view that reflexes, for example, are degenerate forms of acts that were once "voluntary" and attended by a high degree of consciousness. Even so careful a writer as Titchener allows himself the use of analogies which leave the impression on the mind of the reader that he is arguing for a mild form of the lapsed intelligence theory. The unconscious movements of man today, he says, are the descendants of past conscious movements. He goes on to argue his case.

"There is, first of all, the argument from the analogy of the individual lifetime. We learn to swim, to bicycle, to typewrite, to play a musical instrument, with conscious intent and with a constant accompaniment of consciousness; later on, if we practise enough, we do these things unconsciously. If however, what are called 'voluntary actions' may degenerate into 'secondary reflexes' in the course of a few weeks or months or years, it is at least possible that the ingrained physiological reflexes may have a conscious ancestry in the history of the race."[1]

[1] Titchener, E. B. A Textbook of Psychology. p. 452.

It is possible that Titchener does not mean to press his analogy so far; but there are others enough who do. Darwin writes: "It seems probable that some actions which were at first performed consciously, have become through habit and association converted into reflex actions, and are now so firmly fixed and inherited, that they are performed, even when not of the least use. . . . It is scarcely credible that the movements of a headless frog, when it wipes off a drop of acid or other object from its thigh, and which movements are so well co-ordinated for a special purpose, were not at first performed voluntarily, being afterwards rendered easy though long-continued habit so as at last to be performed unconsciously, or independently of the cerebral hemispheres."[2]

It is natural enough that, as knowledge of the characteristic methods of animal learning increased, attempted explanations of the inheritance of acquired forms of behavior which appealed to any sort of deliberation or choice or "conscious intent" on the part of the organisms who first acquired those acts should have fallen into discredit, and the whole theory with them.

But that it is not necessary to hold to any form of the lapsed intelligence theory, to believe that innate forms of behavior were originally voluntary, or that they were attended in their development by any sort of consciousness at all, was demonstrated in principle by Herbert Spencer more than half a century ago. Consider, he says, the case of a simple aquatic creature with rudimentary eyes. Such a creature can be affected by opaque bodies moving in the water only when they are close at hand. But just because they are near by when glimpsed, the chances are high that they will soon afterward come into contact with the organism. In such cases, sight is "little more than anticipatory touch," the visual impression is habitually followed by a tactual one. Now tactual impressions are typically followed by contractions, so that "there constantly occurs the succession—a visual impression, a tactual impression, a contraction." What will then happen is that "the several nervous states produced will become so consolidated that the first cannot be caused without the others following—the visual impression will be instantly succeeded by a nervous excitation like that which the tactual impression produces, and this will be instantly succeeded by a contraction There will then occur a contraction in anticipation of touch." The sequence, originally sight-touch-contraction, be-

[2] The Expression of the Emotions in Man and Animals. pp. 39-40.

comes sight-contraction. The process may then go on producing still more complicated mechanisms, and the effects of such individual modifications Spencer supposes to be inherited. Thus complicated innate forms of behavior arise, not by a lapse from conscious and deliberate forms, but by the compounding of simpler elements in a fashion which is essentially automatic.[3]

Spencer, to be sure, was dealing with hypothetical processes in hypothetical organisms. Actual evidence of the occurrence of such processes was lacking; the explanation seemed speculative, and lacked a strong appeal. Its sufficiency is rejected by Wundt, for example, outright. But, stripped of its outworn modes of expression, stated in modern terminology, we have come to see that Spencer's account of the process as it takes place in the individual organism is not an hypothesis, but a fact. He is, in principle, describing the formation of a conditioned reflex. What he conceived as happening in his aquatic organism is exactly what does happen when a dog comes to react to a light or tone by the secretion of saliva, because he has been presented time after time with the tone or light stimulus simultaneously with the presentation of food which originally serves to excite the salivary secretion. It is again what happens when a human subject comes to react, as in Watson's experiments, to the tone of a bell by jerking his finger away from an electrode because auditory stimulus and electrical stimulus were given for a time in association with each other.

In the light of our present knowledge of such phenomena, there is no need of assuming that it is necessary, in order to show that modifications of behavior are transmitted, to suppose that innate responses as they appear today are the degenerate descendants of more complex and "voluntarily" acquired activities. The conditioned reflex affords a far more adequate and comprehensible method of explaining the origin of such reactions in the race.

There is in particular one class of reflexes which lend themselves admirably to interpretation as the results of just such a formation of conditioned reflexes in ancestral individuals and their transmission to following generations. The reflexes referred to are the glandular secretions which occur as elements of the responses to various emotional situations. We may therefore begin our discussion with a concrete illustration of one such secretion, and a consideration of the way in which it may be supposed to have arisen.

[3] Spencer. Principles of Psychology. 3d ed., pp. 435-440.

In Cannon's remarkable discussion of the bodily changes in fear, pain, and rage, he has demonstrated that the secretion of adrenin under the influence of emotional states "plays an essential rôle in calling forth stored carbohydrates from the liver, thus flooding the blood with sugar; it helps in distributing the blood to the heart, lung, central nervous system and limbs, while taking it away from the inhibited organs of the abdomen; it quickly abolishes the effects of muscular fatigue; and it renders the blood more rapidly coagulable."[4] Each of these responses is, as he goes on to show, useful *to an animal undergoing severe muscular exertion and bodily injury*. The increased blood sugar increases muscular energy; the value of the shift of blood to the heart, lungs, central nervous system and limbs, the abolition of the effects of fatigue, is obvious, as is that of the increased rapidity of coagulation to the injured animal. The whole picture is thus that of an animal preparing himself for a great muscular excitation and possible injury which he is to be called upon to undergo.

But how may such a state of affairs have arisen? The clue seems to be furnished by Cannon himself a few pages further on. For it has been demonstrated by several observers that a moderate degree of asphyxia, such as that resulting from severe exertion, brings about the secretion of adrenin with all its attendant phenomena. In all this, says Cannon, we have a mechanism by which, when the actual stress comes, the changes already initiated by the anticipatory stimulus are continued and augmented. This is, to be sure, the state of affairs in the organism today, but is it not at least thinkable that the secretion of adrenin occurred originally in the race in response to the increase of carbon dioxide attending actual exertion? That the assumption is not incredible seems to follow from the further fact that Cannon is inclined to explain the phenomena of "second wind" by the arousal of the same mechanisms. "According to this explanation 'second wind' would consist in the establishment of the same group of bodily changes, leading to more efficient physical struggle, that are observed in pain and excitement." (p. 210.)

Now 'second wind' is a phenomenon that occurs whether previous emotional excitement has been present or not; the secretion takes place in response to the actual exertion, its stimulus being the presence of the physiological condition sensed as distress in breathing, etc. It is our contention that

[4] Cannon, W. B. Bodily Changes in Pain, Hunger, Fear and Rage. pp. 184-185.

this represents the primitive state of affairs; that, in the race, the original stimulus to adrenin secretion was the exertion itself, not its "anticipation."

To start, then—since we must start somewhere—with an organism already possessing such a reflex mechanism as we have supposed. The mere presence of the object with which the organism had violently engaged in combat, or from which it had escaped in flight, would naturally come after a time to cause the responses which at first were induced by actual contact—conditioned reflexes, that is, would be formed, and the adrenin be secreted when the terrifying or anger-arousing object appeared. Such experiences are common enough under primitive conditions for the conditioned reflex thus established to become deeply ingrained in the nervous system of the animal, and are widespread enough to be common to the members of the race as a whole. The origin of the present mechanism for the secretion of adrenin was thus purely automatic, on the reflex level, and as a conditioned response to common and intense situations—the presence of fear and anger-provoking objects. Such a beginning as the result of individual experience offers a simple and clear explanation of its occurrence in existing forms.

The phenomenon with which we have been dealing does not stand in isolation. Crile holds that it is possible to stimulate the thyroid, adrenals and hypophysis *only* through the "distance ceptors"—that is, by what we may call "anticipatory" stimuli. "According to our observations, no amount of physical trauma inflicted upon animals will cause hyperthyroidism or increased adrenalin in the blood, while fear and rage do produce hyperthyroidism and increased adrenalin."[5] Crile, to be sure, makes too broad a generalization, as Cannon has conclusively shown that, in the case of the adrenals, stimulation of the sciatic nerve such as would, in the normal animal, be extremely painful, does cause adrenal secretion—the presumption is naturally that in the normal animal stimulation of the appropriate "contact ceptors" would produce the same response. (Op. cit. pp. 59-62.)

But it is none the less a significant fact that stimulation of the distance receptors seems to play a predominant rôle in exciting these glands of internal secretion. It is exactly the state of affairs that we should expect to find if such phenomena had, in the race, arisen as conditioned reflexes; as responses once given to contact, but now to the mere presence of significant objects.

[5] Crile, G. W. The Origin and Nature of the Emotions. 1915, p. 133.

Crile himself does regard them as the results of ancestral life-experiences; his term is "phylogenetic associations;" but he nowhere puts himself on record as to the definite method by which, in the primitive individual, he supposes them to have arisen; whether as lapses from more complex forms of behavior, or by means of the formation of such conditioned reflexes as we have been supposing. He contents himself with such general statements as that "their origin is in the operation of the great laws of evolution." (p. 107.) His general positions, however, are such as to imply that in his opinion they did arise in the automatic fashion described.

But such glandular secretions are not the only phenomena of emotional response which may be explained as having arisen by the method which has been suggested. We may, with Crile, go further. His general thesis as to the significance of the emotional pattern as a whole is as follows. The presence of emotional responses means the integration of the body for the performance of the appropriate acts—the racially adequate responses of combat, etc.—but these, because of the influence of the civilized environment are not carried through. "When our progenitors came in contact with any exciting element in their environment, action ensued then and there. There was much action—little restraint or emotion. Civilized man is really in auto-captivity. He is subject to innumerable stimulations, but custom and convention frequently prevent physical action. When these stimulations are sufficiently strong but no action ensues, the reaction constitutes an emotion. A phylogenetic fight is anger; a phylogenetic flight is fear; a phylogenetic copulation is sexual love, and so one finds in this conception an underlying principle which may be the key to an understanding of the emotions and of certain diseases." (Op. cit., p. 76.) Stimulation of the "distance ceptors" by given objects arouses the responses originally made in the race when the "contact ceptors" were stimulated by the same objects. "Sight, sounds, and odors are symbols which awaken phylogenetic association. If a species has become adapted to make a specific response to a certain object, then that response will occur automatically in an individual of that species when he hears, sees, or smells that object." (p. 135.)

Our concern is merely with that phase of Crile's theory which ranges the motor phenomena of emotional expression with the glandular responses already discussed. These, too, may be conceived as responses originally given to actual and stressful contact, but now to the anticipatory situations which,

it may be assumed, were at first relatively indifferent to the organism. The responses of our skeletal muscles at the sight of a snake, then, are those once given by our ancestors to its attack, the muscular tensions of anger, like the adrenal responses, are the original responses to physical contact.

This general point of view is by no means new. It will be remembered that Darwin's explanation of many forms of emotional expression by the principle of "serviceable associated habits" is in principle the same: "Certain complex actions are of direct or indirect service under certain states of the mind, in order to relieve or gratify certain sensations, desires, etc., and whenever the same state of mind is induced, however feebly, there is a tendency through the force of habit and association for the same movements to be performed, though they may not be of the least use. Some actions ordinarily associated through habit with certain states of the mind may be partially repressed through the will, and in such cases the muscles which are least under the separate control of the will are the most liable still to act, causing movements which we recognize as expressive."[6] Others have made use of the same explanation; Stanley, for example, in his "Evolutionary Psychology of Feeling," regards fear as "anticipatory pain;" its origin was in the realization as a result of past individual experience that various situations meant the approach of pain. The same attitude is implied in much of Hall's work; and it is indeed impossible to hold that emotions are to be explained as the results of modifications of ancestral behavior without holding to the theory in some form.

But to make such general statements as that emotional responses have arisen as a result of "association" and "habit," leaves the question of the mechanisms involved still vague and unsatisfactory. Crile has worked out the idea of such "phylogenetic associations" more definitely than any one else. His discussions are free from many of the vague expressions which Darwin, in the light of the knowledge of his day, was forced to use. He has, moreover, increased the sweep of the theory by bringing under it such glandular phenomena as those mentioned above; responses which Darwin explained by the quite different principle of the "direct action of the nervous system." (Op. cit., p. 68.) And, as mentioned above, his whole attitude is such as to make it seem probable that his conception of the origin of the phenomena is that they arose in the same mechanical fashion that we have assumed in using the term "conditioned reflex."

[6] The Expression of the Emotions in Man and Animals. p. 28.

It is just this conception of the origin of the phenomena on the reflex level which frees the theory from the vagueness with which it is usually stated.

Assuming, then, that such phenomena did first arise as conditioned reflexes, the physiology of their formation seems, in principle, clear. Suppose, for example, that the organism is engaged in combat. He is being stimulated not only by the teeth and claws of his adversary, but at the same time by his form, odor, and the sounds which he may emit. The stimuli from tooth and claw have the right of way; they are of the fundamental sort which, as Sherrington has shown, gain possession of the final common paths. Impulses from the receptors of eye, nose, and ear, will in such a case ally themselves with the more fundamental impulses, they will drain into the same final paths. Thus the connections from eye or nose stimulated by the enemy, to laboring muscle and secreting gland will become of lessened resistance, and after one or several experiences of like nature the stimulation of eye or nose will result in the muscular and glandular responses. We may go still further. A threatening growl, for example, though not heard during the combat itself, would, if a typical preliminary to combat, come to evoke the responses. For the mechanisms aroused by the growl would still to some extent be active in the immediately following combat, and would drain into the paths of the predominant mechanisms, with a consequent lowering of resistance as before. It is as simple to explain such phenomena as racially conditioned reflexes, as it would be absurd to suppose that adrenals and the rest somehow, at first under the influence of "the will" and later, under that of "habit," came to secrete in response to mental states called "fear" or "anger."

To show that it is thinkable that such responses may have arisen as results of the formation of conditioned reflexes in individuals is naturally not to convince the sceptic that they did actually so arise. But there is one fact about the emotional responses, muscular and glandular alike, which is worth noting in this connection. It is that, while such responses are eminently adaptive, they are adaptive, not so much in the situations in which they now occur, as in situations which, in the history of the race, typically followed. That some of these responses are not only, in the situations in which they occur, not serviceable, but that they are actually harmful unless those situations are followed by the racially appropriate acts, has been shown in some detail by Crile, in the work cited above.

It is easy to explain these facts on the basis of our proposed theory. But, those who deny the possibility of the inheritance of acquired characters would naturally retort, it is also possible to explain them without resorting to such hypotheses. For, once they appeared as variations, they would be useful to a degree in preparing the organism for the following exertion; and their harmful effects in man today are due to the changes in his life which civilization has wrought. More, the view is growing that all variations need not be useful in order to be preserved; such responses might be retained once they appeared as variations (mutations) if only they were not harmful enough to be a handicap to the animal, as obviously, under primitive conditions, they were not.

But if the responses did appear as variations, either continuous or discontinuous, it was naturally as variations from some not too different state of affairs already existing, and the argument lies near that this pre-existing state of affairs was that in which the responses were given to situations involving actual physical exertion. If this is the starting point, the question is, did the transfer of the responses to the anticipatory situations come about through variations or through the formation of conditioned reflexes? The more we realize the complexity of the emotional responses, the less credible does it seem that element after element, or the whole integration, should so have been transferred through variation. That one, or several responses might, is understandable; but that the whole picture of an organism under great bodily stress should have been projected, so to speak, by variation, on another and anticipatory type of situation, not once but many times, is hardly to be believed, especially when the simpler alternative is available.

Every element in the classic description of the fear responses which Darwin has given, spells in words of one syllable struggle, flight and the exhaustion which results. As he himself says: "Men, during numberless generations, have endeavored to escape from their enemies or danger by headlong flight, or by violently struggling with them; and such great exertions will have caused the heart to beat rapidly, the breathing to be hurried, the chest to heave, and the nostrils to be dilated. As these exertions have often been prolonged to the last extremity, the final results will have been either prostration, pallor, perspiration, trembling of all the muscles, or their complete relaxation and now, whenever the emotion of fear is strongly felt, though it may not lead to any exertion,

the same results tend to reappear, through the force of inheritance and association." (Op. cit., p. 307.) Is it not almost unthinkable that continuous variations or mutations should have built up such an integration, in a situation in which it is of little value and even at times of great harm to the organism—an integration which moreover copies so perfectly the picture of responses to a second sort of situation (flight and exhaustion), which in the life histories of "numberless generations" closely followed on the sort of stimulation to which the fear responses are now given? Is it conceivable that such copies should be evolved by variations and that not in one emotion, but in many, and down to minute detail in case after case? To affirm this savors of a mysticism that is almost Bergsonian. Truly the facts of emotional response offer but a slippery footing to advocates of the all-sufficiency of variation. Darwin himself seems to have seen this clearly; it is not by accident that in the "Expression of the Emotions," explanations by variation have retreated to the background.[7]

We must not forget, in considering the possibility of the inheritance of acquired modifications of behavior, that in one case such a transmission seems to have been shown unquestionably. This is the case reported by Kammerer, dealing with his work with the obstetrical toad.[8]

Kammerer compelled the animals, whose eggs are normally laid on land and taken up and carried about for a time by the male, to breed in the water, which normally they rarely

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[7] The writer's picture of the transmission of such responses would be this: The responses made by the animal under conditions of stress depend obviously on the nature of his "action system" (including glandular reactions). The conditioned reflex, when formed in the individual, arouses the whole pattern of responses which the animal originally gave to the stressful situation. The resulting modification of the germ plasm is of such a nature that, though possibly only one substance may cause it, the whole pattern of responses will be reproduced in following generations. (One may remember here the phenomena of concomitant variations). The modification may at first be slight; the tendency to give the responses in anticipatory situations by the offspring, feeble. The life experiences of these organisms would strengthen the tendency, and after several or many generations it would gain the strength which it now has. As, in the course of evolution, the animal's action system changes, his responses to stressful situations change, and new elements are added by his experiences to his emotional responses; the pattern thus becomes different in the individual, the modification of the germ plasm is different, and the resultant behavior in the offspring is different.

[8] Kammerer, P. Vererbung Erzwungener Fortpflanzungsanpassungen.—III. Die Nachkommen der nicht brutpflegenden Alytes obstetricans. Arch. f. Entwick., 1909, 28.

visit. After several seasons of compulsion, the animals came to breed in the water, even when the compelling stimuli were withdrawn. The broods reared from the eggs of this generation showed (after the first), increasing modifications of their breeding instincts in the direction of the modifications acquired by their parents. The later broods would breed the first season in the water, the second on land without the characteristic taking up of the eggs by the male, and only in the third breeding season did they revert to the normal ancestral behavior. In this case, a continued experience of one generation was sufficient to modify the behavior of a second generation. The acquisition of the modified form of behavior in the first generation has about it nothing "voluntary" or "deliberate," nothing that suggests analysis of the situation. Kammerer's method of causing the animal to breed in water was by the use of high temperatures at the breeding season. To such stimulation the animal responded as his innate mechanisms impelled—he sought the water reflexly. The situation "a high temperature" and the situation "a certain physiological state characteristic of the breeding season" occurred several times together, and were invariably followed by the response "going to the water." It naturally followed that the physiological state characterizing the breeding season came to cause, when present alone, the water-seeking response. The mechanism is again that of the conditioned reflex.

It seems significant that this modification, so promptly transmitted, was a modification in an element of a behavior-complex (breeding behavior) that is obviously characterized by deep and widespread bodily reverberations. When we come to know even as much of the bodily changes which characterize the operation of the instinctive breeding-mechanisms as we do of those characteristic of fear and rage, we will in all likelihood see that no organ or tissue of the body is exempt. How widespread are even the superficially observable responses in such a case is well shown by Craig, who, in speaking of the behavior of the female ring-dove who has thrown herself into the mating and laying attitude, says "When she does so, her whole organism is affected. Her position in standing and her carriage in walking are greatly altered—her whole bearing shows intense emotion, not violent, but deep."[9]

It has been argued above that it is in cases characterized by just such a sort of behavior that the evidence for the trans-

[9] Craig, Wallace. The Stimulation and the Inhibition of Ovulation in Birds and Mammals. Jour. of Animal Behavior, 1913, 3, 215.

mission of acquired modifications is strong. The responses now emotional were originally given, we have assumed, to situations which, like that with which Kammerer was dealing, involved the widest sort of bodily resonances. The organism was, *as a whole,* integrated for the combat he was undergoing, the injury he was receiving, the flight he was undertaking.

In the breeding responses, again, the animal is integrated for one end alone. May we not, then, venture to assume that the cases in which it is most likely that acquired modifications of behavior have been inherited are those *in which the situations to which the responses were originally given were attended by a wide degree of integration of the organism as a whole in the service of some fundamental task.* That such situations would in the individual be highly favorable for the formation of deeply ingrained conditioned reflexes is obvious. And that such conditioned reflexes themselves involving the same thorough integration would be more likely than those of a milder nature to modify the germ-plasm seems evident.

We have so far attempted to show that, in cases of emotional responses, an origin as conditioned reflexes offers a simple explanation of the phenomena as they actually appear, while their origin as slight variations or as mutations is difficult to explain, since they do copy so closely life experiences of the animal in typically associated situations. Kammerer's case of the actual transmission of an acquired modification of behavior shows the mechanism of the conditioned reflex in the individual, in a situation involving integration of the organism for a fundamental purpose, and we have been led to the statement that conditioned reflexes formed under circumstances which involve such integration—as in the conditions of stress from which emotional responses derive, or those involving the operation of fundamental instincts—would be in a favorable position to bring about modifications of the germ-plasm.

But such a statement remains unconvincing unless it is possible to show that a means exists by which such conditioned reflexes may modify the germ-plasm, while others of different type may not. There is, as a matter of fact, a recent theory of the mechanisms involved in the transmission of acquired characters which lends support to our assumption. Cunningham has contended that the germ-plasm may be modified through the action of hormones secreted by various bodily structures and carried by the circulation to their destination. There is evidence that the germ-cells affect the soma in such a way, and it is only natural to assume that the

process may be reversed.[10] Now conditioned reflexes which have arisen in the race from integrations of the fundamental nature we have been describing do as a matter of fact involve as a part of their nature the activity of those cells which produce such internal secretions. Hence, once acquired by the individual, they are in a strategic position so far as modifications of the germ-plasm are concerned. They are characterized by the production of internal secretions; conditioned reflexes of a less fundamental sort acquired by the individual are less likely to be so accompanied.[11]

The theory proposed above that inheritance of acquired modifications of behavior is most likely to occur in the case of those experiences which the individual meets by an intense and thorough integration may also serve to show why certain types of racial experience are not inherited but must be acquired afresh by each generation. It weakens, for example, the force of evidence of the sort cited in the following quotation.

"It is remarkable that certain evidence from human psychology has failed to receive attention in all these long debates. Human life offers a favored case for transmission of an acquired trait where transmission has clearly failed. The congenitally blind from eye defects do not have visual images of the sun, stars, or any other of the permanent objects of the natural world, yet their ancestors for at least hundreds of generations, save in the case of those lacking in visual images, had such images again and again. If the hourly experiences of hundreds of ancestral generations do not become a part of inborn equipment, we could hardly expect anything to do so."[12]

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[10] Cunningham, J. T. The Heredity of Secondary Sexual Characters in Relation to Hormones. Arch. f. Entwick., 1908, 26.

[11] It is necessary, in making such an assumption, to remember that Cannon has argued that the glandular responses, and the activities of the autonomic system in general, are too similar in the various emotions to serve as a basis for the actual differentiation of these in the individual. Their distinguishing features, he holds, are furnished by the central nervous system. (Cannon, W. B. The Interrelations of Emotions as Suggested by Recent Physiological Researches. Am. Jour. Psy., 1914, 25, pp. 256-282.) Angell has replied that, while single elements may be much the same, the patterns may be very different. (Angell, J. R. A Reconsideration of James's Theory of Emotion in the Light of Recent Criticisms. Psy. Rev., 1916, 23, pp. 251-261.) This may of course be true of the glandular as well as of the motor phenomena involved. There is also the possibility of slight chemical differences in the secretions in the various responses, and of the presence of other secretions as yet unknown. We must remember that systematic work in this field is in its infancy.

[12] Thorndike. The Original Nature of Man. p. 234.

Thorndike's case, on our hypothesis, is not crucial at all. For the permanent phenomena of the world are not faced by organisms with a high degree of integration. Nor do the responses which, in the course of individual experiences, are built up as images of those objects, involve the taking of such definite and integrated bodily attitudes. Even if it became necessary to adopt the extreme position of Watson, that visual images are in reality nothing but the direct sensory results of slight motor adjustments, it is evident that the motor phenomena involved are not of such a nature as by our hypothesis we would expect to be able to bring about modifications in the germ-plasm. On the other hand, man *does* inherit responses to temporary and violent disturbances in natural phenomena, in the various sorts of fear responses, for example. In Hall's returns, fears of this sort rank high[13] and it is easy to see how the organism at first reacting intensely to the effects of such disturbances on itself and its fellows, could come by the formation of racially conditioned reflexes to react as it now does to their mere appearance. We do not, to be sure, react by getting images of their effects, but in the more natural way of reproducing the confused mental states and the bodily attitudes originally produced by those effects. There is no disposition to discuss here the question whether man ever does inherit responses to ideas or by ideas. A consideration of the confusing evidence may be reserved for another paper. But it is evident enough that ideas of permanent natural objects lack the essential physical bases which would make their inheritance likely. Our contentions may be summarized as follows:

1. The supposition that man's innate responses have arisen as the results of acts performed at first, in the race, in a deliberate, or voluntary fashion, with analysis of the attendant situations, with "conscious intent," and subsequently mechanized by the individual and so transmitted, is unlikely, from what is known of animal behavior.

2. There is however, another method by which modifications of behavior do take place in organisms which can easily account for the origin in ancestral organisms of many forms of behavior which we are supposing to have been transmitted. This method is that of the formation of conditioned reflexes.

3. Glandular responses such as those given in strongly emotional situations become easily comprehensible if they are viewed as conditioned reflexes which, once set up in ancestral organisms, were transmitted.

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[13] Hall, G. S. A Study of Fears. Am. Jour. Psy., 1897, 8, pp. 147-249.

4. Not only such glandular reactions, but the whole pattern of emotional response, is easily understandable on such a hypothesis. It is almost unthinkable as a copy of the responses to another type of situations (actual flight, combat, etc.), arising from variations.

5. Kammerer has shown that modifications in the breeding behavior of one species of toad—behavior involving deep and widespread bodily resonances—are transmitted.

6. It seems possible, then, to argue that modifications in forms of behavior attended by intense and thorough integration of the organism are likely to be inherited.

7. Such a theory receives at least partial support from the "hormone theory" brought forward by Cunningham.

8. The theory propounded in (6) also serves to explain why certain sorts of ancestral experiences are not inherited.

It remains to note that this paper is not an attempt to explain all sorts of innate behavior as having arisen by the method suggested. There is no reason to deny the origin of perhaps much of man's original behavior in variations, whether of the continuous sort supposed by Darwin, or the discontinuous mutations which are now so much stressed. Both variations and the transmission of acquired modifications seem to have been at work; one theory does not exclude, but supplements, the other.

And, finally, the view of the "original nature of man" to which such a theory leads makes it far richer than advocates of the all-sufficiency of variation have supposed. But the discussion of such implications of the theory may be reserved for another occasion.

THE EFFECT OF SOUND DISTRACTION UPON MEMORY[1]

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I. Introduction

The writer of this article has recently published[2] a monograph reporting the results of an experiment designed to test the effect of sound disturbance upon continuous complex reaction to visually presented material. This experiment showed clearly that the extra resistance to free reaction which the distraction set up had the effect of causing the subjects to exert extra energy to overcome the disturbance, and to aid themselves by articulating the letters and numbers that were involved as intermediate steps in the reactions. As a result of the use of extra energy and the aid of articulation the subjects were enabled after a brief period of adaptation to make as good time and to do as accurate work during noise disturbance as during quiet.

Since noise causes an extra discharge of energy and the adoption of some scheme to overcome disturbance it must follow, either that noise has a dynamogenic effect, or that there is some general tendency to meet increased resistance to the customary response with an extra output of energy to overcome the increased resistance. Support for the latter conclusion was found in a weight lifting experiment, where it was found that a change in the size of the weight was accompanied by a change in the force exerted, despite the fact that the subjects were instructed to do their best all the time. Hence the conclusion was submitted that any change in the resistance offered to the maintenance of the pace adopted by the reagent causes a change in the output of energy.

Professor Woodworth has recently performed some experiments which point to a similar conclusion in the case of memorizing lists of word paired-associates. From a number of different experiments with various class groups he found that the percentage of material retained after two days increased

[1] From the psychological laboratory of Columbia University.

[2] The Overcoming of Distraction and Other Resistances. *Archives of Psychol.,* 1916, No. 35.

with the length of lists used in the presentation. After informing the class of the size of the list to be given, the pairs were read. Immediately the first word of each pair was again presented, the class if possible recording the correct associate, and each failure being followed by a prompting with the correct word. After three repetitions in this fashion lists of other lengths were presented. After an interval of two days retention was tested for all the lists. The fact that all the conditions of the experiment were the same with the exception that the lists were of different lengths shows that in some way the response varied with the length of the list.

One explanation that presented itself was that the long lists caused the subjects to exert greater energy and to find more lasting bonds than in the case of the shorter lists. As a bit of collateral evidence for this Professor Woodworth suggested that we examine the effect of noises upon retention. Unfortunately for a comparison of his results with those of the experiment to be presented, the apparatus that was used forced us to use material different from that used by him; this with the fact that our results came out differently from what was expected prevents their being used to any great extent for the purpose first intended. They do however supplement our previous work and show that, while one opposes extra effort against increased resistance to his set pace of activity, there are some mental processes that lose in the presence of distraction in spite of such increased effort.

II. The Experiment

The general plan of the experiment as well as the nature of the material used was suggested by Professor Woodworth. In general the procedure followed was the automatic presentation of a list of ten paired associates at two-second intervals, the associates consisting of a three letter word and a number of a single digit. After the first presentation of the series, a word was exposed alone to which the subject responded by pressing a key corresponding to the number he thought was associated with the word in the first presentation. (There were ten keys in all, numbered in order 1, 2, 3, 4, 5, 6, 7, 8, 9, 0.) The pressure of the key caused the correct number to appear, to which the subject again reacted by pressing the corresponding number. This reaction caused the next word to appear, and the process was thus continued until he had responded three times to each word in the series. Four such lists were used with each subject, two in quiet and two in noisy conditions.

While the learning was in progress graphic records were taken of the time of each reaction, of the accuracy of the responses, of the subject's breathing, and of the amount of pressure he exerted upon the keys in reacting. Two days later the subjects were asked to recall as many of the words used in the experiment as possible; and after the recall test they were given the forty words that had been used, mixed with forty new words and they were asked to check those they recognized as having been used in the experiment. They were then given two chances to respond to each word with the number that had been associated with it in the presentation.

The apparatus was a modification of that used in our previous work on distraction.[3] The material to be presented was placed upon a disc which moved one sixteenth of a revolution every time the operating magnet received an electric current. Except for a small part the disc was hidden behind a movable screen. At each revolution of the disc a contact was made which caused (by means of an escapement movement[4]) the screen to move horizontally thus exposing a new part of the horizontal segment of the disc. As the disc moved a distance of 22.5 degrees at each reaction, there was a segment of 22.5 degrees which could be subdivided and used for varying exposures in the successive revolutions. In this case the segment was divided into nineteen parts thus enabling us to give a series of 304 exposures.

The first presentation of each list was controlled by a seconds pendulum which made contact every two seconds and operated by such contacts the exposure apparatus. This pendulum also made a connection every second and through this contact a time-line magnet was operated. Each key upon which the subject reacted was connected with a recording magnet. The keys as a whole were equipped with a pneumatic apparatus to record the pressure. All the recording material was in a separate room so that the subject was alone during the entire course of the work.

The distractions used were three in number. A graphaphone was played during each noisy period. At the same time a large fire gong and a buzzer placed behind the subject were kept sounding alternately at varying intervals of change.

The following lists present the associates that were used, the order representing that of the first presentation. In each

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[3] *Ibid.*, 11-21.

[4] Three different adaptations of the escapement principle are now being used for exposing material in the Columbia Laboratory, and in every case with very satisfactory results.

succeeding presentation the order was changed so that serial associations could not factor.

ale - 9	oak - 9	gem - 7	arm - 5
bag - 6	pen - 8	roe - 1	bog - 1
ear - 7	son - 4	oil - 3	cat - 3
fir - 1	sty - 6	inn - 4	din - 8
hoe - 2	urn - 1	hay - 5	egg - 9
jew - 8	war - 3	law - 8	hat - 2
mud - 5	cog - 2	bee - 2	imp - 6
owl - 4	eye - 5	car - 0	joy - 7
pun - 3	fop - 7	fad - 6	lye - 0
sap - 0	jaw - 0	wit - 9	peg - 4

The directions given the subject will make clear the details of the presentation. They were given the directions to read before beginning the work, supplemented when they were not clearly understood by oral direction. The written directions were as follows: "Sometime after I leave the room the word 'ready' will appear at the exposure opening. Two seconds later and at two second intervals thereafter will appear a word and a number separated by a hyphen. Study these paired associates carefully and try to remember the number belonging to each word. After ten pairs have been exposed one of the words will appear alone. React to this word by pressing the key corresponding to the number that belongs to it. If you cannot think of the proper number, guess. The pressure of any key will expose the right number; to this you will react by pressing the corresponding key. For example, suppose in the first exposure you saw 'ice-2,' when ice appears alone you press the number you think you saw with it. If you press 3, it will cause the number 2 to appear, and you will then press 2. If you press 2 the first time, you will see by the appearance of the 2 at the opening that you were right and will press it a second time. A new word will appear only on the second reaction. Respond to each word in this manner until you see the word 'REST.' At the appearance of this word wait until another list of associates are exposed, beginning to react at the sight of the first word with no number attached. Always react with the index finger of the right hand."

The recall, recognition and retention tests were made as nearly as possible two days after the learning. For the sake of convenience a few of the subjects were permitted to do these outside the laboratory. In every case four envelopes were given numbered 1, 2, 3 and 4; together with four slips of paper on which were printed the following directions:

Directions for Envelope No. 1

In envelope number 1 you will find a sheet of blank paper. On it write all the words you can think of that were used in the experiment two days ago. (Not including Ready, Rest, etc.). Return the sheet to the envelope before reading directions for envelope number 2.

Directions for Envelope No. 2

In envelope 2 you will find a list of eighty words. Place a check mark after every word that you recognize as having been used in the test. Place a cross after every word that you recognize as not having been used. Place a ? after every word that you cannot place in either of the first two groups. Complete and return the paper to the envelope before reading directions for envelope number 3.

Directions for Envelope No. 3

In envelope 3 you will find a list of the words used in the experiment. Begin at the top and in order place after each word the number associated with it in the experiment. Place some number after each word. If you do not know, guess; but place a ? after each such number. Do not go back and correct any previous judgments. Record the time taken for this test. Return to the envelope before reading directions for test number 4.

Directions for Envelope No. 4

Proceed exactly as in the case of test number 3.

III. Results

Before presenting the results in detail it may be well to emphasize the fact that the material used was of such a nature that the forming of bonds was very difficult. In no case did the subjects report the use of any systematic scheme to aid in the learning. Incidental connections arose such as for one subject "car-o" suggested "Karo" Corn Syrup; "jew-8" was fixed easily by several subjects of jewish blood; "peg-4" by one subject was associated with a chair of four legs, etc.; but the majority of learning was purely rote. If this is borne in mind the significance of the results will be more apparent.

Twenty subjects, mostly psychology students, were used; eighteen were men and two women. For ten the first and third lists were given in quiet and the second and fourth in noise, while for the other ten the first and third lists were given in noise and the second and fourth in quiet. The order

in which the lists were presented was the same for all subjects. In computing the results (if we call one group of ten subjects A and the other group B) the first period of group A was averaged with the second of group B, the second of group A with the first of group B, the third of group A with the fourth of group B, and the fourth of group A with the third of group B. This method eliminates any chance factor that might have made one list more easily learned than the other and also takes account of practice effect. In viewing the results to ascertain the effects of the distraction only the periods should be compared which have been so combined, that is the first "quiet" column presented in the table may be compared with the first "noise" column and the second "quiet" column with the second "noise" column.

Table I gives the learning records of the twenty subjects in terms of the number of correct responses in a possible thirty, each series being given three repetitions besides the original presentation. If we compare the first noise period with the first quiet we find that two subjects had more correct responses

TABLE I

Learning Record. The Scores Indicate the Number of Correct Responses in a Possible Thirty

Condition	Quiet	Noise	Quiet	Noise
Period	A	B	C	D
Subject *A*	9	6	9	11
B	6	13	6	9
C	17	7	6	8
D	25	22	23	21
E	10	9	17	13
F	12	12	8	11
G	22	22	24	17
H	22	6	20	22
I	5	3	10	7
J	15	9	14	11
Period	B	A	D	C
K	9	6	19	14
L	8	7	15	11
M	10	6	10	7
N	7	4	15	10
O	9	3	14	15
P	5	10	17	10
Q	11	5	11	8
R	17	12	17	10
S	15	3	10	20
T	30	28	27	28
Ave.	13.2	9.6	14.6	13.15
$P.E._m$	1.1	0.96	0.91	0.85
	$P.E._d$ 1.4		$P.E._d$ 1.2	

in the noisy conditions, two the same number, and sixteen less. A comparison of the second two periods shows eight subjects having more correct and twelve less in the noisy than in the quiet. The difference between the averages of the first quiet and noisy periods is 3.6 with a probable error of the difference of 1.4, giving a difference 2.5 as great as the probable error. The difference between the averages of the second quiet and noisy periods is 1.5 with a probable error of the difference of 1.2. This shows that at first the noises were more of a hindrance than later, although the adaptation is not complete in the second period. That the second difference is so much less than the first would indicate that adaptation was setting in rapidly and that if the test were continued longer there would be no effect of the noises as far as the learning scores go. We have little doubt but that this would be the case since that is precisely what occurred in our former work, although there adaptation was very rapid. While on account of the adaptation that does show itself the learning scores do not indicate a permanent effect of the noises, we will later present evidence that it has a permanent effect in other respects.

TABLE II

TIME RECORD FOR FIRST RESPONSES. THE SCORES INDICATE THE AVERAGE TIME PER REACTION IN SECONDS

Condition	Quiet	Noise	Quiet	Noise
Period	A	B	C	D
Subject *A*	4.80	3.32	3.78	2.57
B	6.00	3.07	3.10	2.88
C	3.55	2.36	2.57	2.35
D	2.17	2.02	2.13	1.86
E	3.71	2.08	2.45	2.34
F	3.38	2.01	2.45	2.43
G	3.96	2.70	2.41	2.54
H	2.41	3.01	1.72	1.66
I	2.88	2.17	2.26	2.57
J	1.62	1.47	2.04	1.95
Period	B	A	D	C
K	2.19	2.90	2.07	1.91
L	3.17	4.13	3.09	3.43
M	5.00	7.42	5.18	5.38
N	3.12	4.22	2.75	3.11
O	3.17	3.54	2.27	2.10
P	3.15	3.10	2.37	2.90
Q	2.58	2.52	4.18	2.45
R	2.27	2.81	2.32	2.47
S	4.03	5.17	3.72	3.78
T	2.42	2.26	2.02	1.76
Ave.	3.28	3.11	2.74	2.62
P.E.m	0.15	0.17	0.12	0.11
	P.E.d 0.23		P.E.d 0.16	

TABLE III

TIME RECORD FOR SECOND RESPONSES. THE SCORES INDICATE THE AVERAGE TIME PER REACTION IN SECONDS

Condition	Quiet	Noise	Quiet	Noise
Period	A	B	C	D
Subject *A*	1.72	1.77	1.35	1.35
B	1.66	1.47	1.67	1.37
C	1.30	1.35	1.28	1.21
D	1.17	1.03	1.02	.91
E	1.92	1.35	1.19	1.22
F	2.03	1.38	1.67	1.34
G	1.84	1.18	1.15	1.32
H	.96	1.14	.71	.70
I	1.45	1.28	1.19	1.27
J	1.25	1.19	1.06	1.03
Period	B	A	D	C
K	1.17	1.50	.97	1.05
L	1.57	2.42	1.37	1.41
M	2.33	3.82	2.03	2.80
N	1.92	3.32	1.73	2.08
O	1.54	2.07	1.02	1.15
P	1.82	1.72	1.32	1.54
Q	1.44	1.95	1.53	1.34
R	.92	1.41	.90	1.14
S	1.40	1.97	1.29	1.11
T	.86	.90	.63	.58
Ave	1.51	1.71	1.25	1.24
P.E.m	0.06	0.10	0.05	0.05
	P.E.d 0.12		P.E.d 0.07	

The time taken in reacting shows no significant change due to the noises. It will be remembered that there were two reactions to each word, the first was by pressing the number which the subject thought belonged with the word exposed, the second was to react to the correct number which the first reaction caused to appear. We shall designate the first pressure as the first response and the reaction to the correct number as the second response. The two were computed separately; those for the first responses appear in Table II, while those for the second responses are given in Table III. It is evident that in the case of the first responses there is no loss in speed as a result of the distractions, what little difference there is lies in the direction of greater speed during the noise periods. In each comparison twelve subjects show faster time and eight slower in the noise periods. The difference between the averages of the noise and quiet periods falls within the range of pure chance.

The explanation of the lengthening of the time of the second

responses in the first noise period is not apparent, but as the difference between the average of this period and the first quiet period is only slightly greater than the probable error of the difference it is likely a chance difference. A comparison of the times of the second quiet and noisy periods certainly indicates no effect of changed conditions; ten subjects made slower time, one the same and nine less, while the averages are almost identical.

TABLE IV

Breathing Ratios. The Scores Indicate the Average Ratio Found by Dividing the Expiration Time by the Inspiration Time

Condition	Quiet	Noise	Quiet	Noise
Period	A	B	C	D
Subject *A*	2.28	2.62	1.97	2.50
B	2.19	2.69	2.73	2.42
C	1.41	1.28	1.41	1.38
D	1.27	2.51	1.44	2.49
E	1.94	1.87	2.85	2.22
F	2.03	1.61	2.01	1.73
G	1.53	1.30	1.32	1.59
H	2.32	2.67	2.07	2.04
I	1.78	1.96	2.19	2.39
J	1.56	1.67	1.52	1.65
Period	B	A	D	C
K	2.16	2.34	3.10	2.15
L	2.86	2.28	2.54	3.80
M	3.70	2.34	2.91	6.24
N	1.68	2.04	2.00	2.04
O	1.97	1.87	1.75	1.82
P	3.10	2.56	2.37	2.73
Q	1.47	1.84	1.75	2.42
R	1.62	2.41	1.53	1.64
S	2.62	3.97	2.24	2.83
T	2.69	2.25	2.05	2.52
Ave.	2.11	2.20	2.09	2.43
$P.E._m$	0.09	0.08	0.09	0.11
	$P.E._d$ 0.12		$P.E._d$ 0.15	

In our previous work on distraction it was found that with the task we used articulating served as a great aid in the accomplishment of the work; consequently the subjects showed a strong tendency to use this means to help overcome the disturbing effect of the noise. We recorded this tendency through the comparison of the expiration and inspiration times. When one speaks, or moves even slightly the organs of articulation, the inspiration time is shortened and the expiration time lengthened. The ratio of the expiration to the inspiration is there-

fore changed and may be used as a scale of the extent of such articulatory movements.

Table IV presents the ratios for our twenty subjects in the two periods with each condition. The first noise period causes no marked difference in the ratio while the second one shows a slight tendency toward an increase in ratio for the noisy period. Fourteen subjects have a larger ratio and six a smaller. It will be seen however that in only a few cases is the increase very marked, and we must conclude that the tendency to articulate in the work of memorizing the material we used is slight. This is not surprising, for an analysis of the process involved shows little help could be gained by speaking the words and numbers in comparison with the help that could be gained in the task used in our previous work. There the subject had to look at the exposure opening where he perceived a letter from the latter end of the alphabet on a background of one of three colors. He then looked below the opening to a code by means of which he translated the exposed letter to one from the beginning of the alphabet. This letter he translated to a number by selecting from three codes one colored similarly to the background of the exposure, and having obtained the number from this code pressed the key; whereupon the exposure was changed and the process repeated. In this complex procedure lip movements, whispering, or speaking aloud aided materially and these helps were used by a large number of subjects. Very slight movements would serve to repeat the word and number of a paired associate and hence we believe the difference in the tasks in the two experiments explains the difference in the results obtained. However even in this test such aids were used to some extent for the difference in the ratios of the last quiet and noise periods is 2.3 times the probable error of the difference.

Another and by no means the least important record taken during the learning period was that of the pressure used by the subjects in reacting upon the keys. The keys were accurately and delicately adjusted and the least pressure upon them was, by a pneumatic connection, recorded upon a smoked drum. As the contacts were of mercury a very light pressure served to make contact.

In Table V are given the average pressures in grams for each of the lists learned, two in each condition for each subject. There is a marked tendency to press the keys harder when noise disturbance is in progress than when surroundings are quiet. Fourteen subjects react with greater pressure in the first noise period than in the first quiet while sixteen

TABLE V

KEY PRESSURES. THE SCORES INDICATE THE AVERAGE PRESSURE IN GRAMS

Condition	Quiet	Noise	Quiet	Noise
Period	A	B	C	D
Subject *A*	395	257	260	188
B	225	325	145	218
C	390	337	235	175
D	460	550	352	593
E	1038	922	825	655
F	362	1135	440	710
G	130	118	140	185
H	572	708	582	630
I	358	325	212	313
J	1165	900	745	625
Period	B	A	D	C
K	522	1000	595	815
L	220	565	187	208
M	475	1425	155	435
N	242	762	180	235
O	115	175	165	168
P	112	168	77	112
Q	147	448	75	197
R	1422	1718	905	1475
S	88	260	43	68
T	207	420	90	100
Ave	432	625	320	405
$P.E._m$	50	67	42	51
	$P.E._d$ 83		$P.E._d$ 66	

react with greater pressure in the second noise period as compared with the second quiet. In each case a comparison of the averages shows a reliable difference, the difference being more reliable in the first than in the second comparison. This is the opposite of the results of the breathing ratios, where the second comparison showed a more reliable difference. These two sets of facts point to the conclusion that the introduction of noise causes the subjects to strain to overcome them, the strain being most marked at the beginning and lasting, although not to such a marked degree, throughout the noisy conditions, giving place to relaxation when they cease. The fact that the breathing changes most in the latter period indicates that it takes some time to adopt articulation as an aid.

In these records of articulation and key pressure we by no means have an adequate check on all the ways in which the subjects oppose the effects of the noises. They do however throw valuable light upon the subject's behavior, light

that could never be obtained from the usual methods of merely recording speed and accuracy. No two subjects were affected alike, as a close study of all the tables will show, and some undoubtedly opposed the noises by reactions upon which we had no check; nevertheless the measures we did get connect with the speed and accuracy scores fairly well. If we take the second periods of quiet and noise, when the subjects had become somewhat adapted to the situation, and compare the different individuals we will see that;—of the eight individuals who made a better learning score in the noisy period only one had neither a higher breathing ratio record nor a higher key pressure record in that period, one had a higher breathing ratio record and a lower key pressure record, three had a higher key pressure record and a lower breathing ratio, while three had both a higher breathing ratio and key pressure score; of the twelve who made a lower learning score in the noisy period nine had both a higher breathing ratio and key pressure score, one had neither and the other two had one score higher and the other lower. This is certainly evidence of wide individual variation. Some in spite of effort and articulation were still unable to make as good a score in the noisy condition as in the quiet, others were able to make as good a score with these aids, while one did so without giving us any evidence as to the means of his so doing.

Now if by any possible means we could get enough simultaneous measures of an individual during an experiment so that every possible reaction in any direction whatsoever could be measured, a composite expression of all these measures would correlate perfectly with the measures of efficiency provided we had a perfect measure of work done per unit of time along with a perfect measure of quality. If then any disturbing factor should be introduced it would either affect the efficiency score; or, if the subject overcame it by some adaptive reaction, the composite figure expressing his reactions *in toto* would be changed. Those individuals who stood highest in the efficiency score under such conditions would show the greatest amount of change in the composite measure of adaptive reactions, those lowest in the efficiency scale would show the least change in the reaction measure. If some outside factor were introduced in an endeavor to ascertain what influence it had and neither score was changed it could be safely inferred that it had no bearing on the task in hand. If this is so, and if in our experiment the noises had any effect, and if the tests we used were at all adequate we should get some sort of correlation between the efficiency

score and the measures of the individual's changes in reactions of an adaptive sort. Since our tests make no boast of being comprehensive we could not expect a perfect correlation.

To get such a correlation we found the amount of each individual's superiority in the noisy period over the quiet; that is, if a score of 5 were made in quiet and 4 in noise the superiority was —1, if the time in quiet was 2.2 seconds and in the noise 2 seconds the superiority was .2 of a second. We proceeded in like manner with the breathing ratio and key pressure scores counting a higher score in the noise period as a plus difference. Having found these differences we arranged the subjects according to their relative ranks as to these differences in each test, the greatest plus difference being one and the greatest minus difference being twenty. The average rankings for each subject were then found for the learning and time, and for ratio and key pressure. In the case of the comparison of the first quiet and noisy periods the correlation between these two average rankings was .35 (P.E. .138), and in the case of the second quiet and noisy periods .57 (P.E. .11). That the correlation is higher in the second comparison is significant. In the first change in situation the adaptive reactions did not factor nearly as much as in the second change, the subjects were more or less bewildered while in the second they settled down to overcome the disturbing effect; hence it would be expected that the correlation would be higher in the second comparison.

That a single measure would not have been nearly so adequate is shown by the fact that, if from the same relative rankings of the differences, taking the comparison of the second periods, we compute the correlation betwen time and key pressure, we find it to be —.05, and that between breathing ratio and amount learned is only —.04, that between key pressure and the average of the learning and time .14 (P.E. .15), and that between the breathing ratio and the average of time and learning in .28 (P.E. .145).

We likewise have evidence that the associative bonds that were formed during the noisy conditions were more superficial than those formed in the quiet periods. In Table VI may be seen the results of the retention test taken after a two day interval. The scores represent the number of correct associations given in two trials, that is, the correct responses in a possible 20. Here in each case we have a lower score for the noisy than for the quiet periods. If we find what per cent the retention scores are of the learning scores in each of the four lists we find for the first list the

TABLE VI

RETENTION OF ASSOCIATIONS. THE SCORES INDICATE THE NUMBER OF CORRECT RESPONSES IN A POSSIBLE TWENTY. SUBJECT M. FAILED TO COMPLETE THIS PART OF THE EXPERIMENT

Condition	Quiet	Noise	Quiet	Noise
Period	A	B	C	D
Subject *A*	3	1	7	7
B	4	4	2	3
C	1	5	7	1
D	7	3	8	4
E	5	5	4	4
F	5	4	3	12
G	9	4	6	1
H	3	1	3	4
I	3	2	4	2
J	3	2	6	3
Period	B	A	D	C
K	3	3	4	3
L	4	4	4	5
M	..	..	..	..
N	3	1	6	5
O	6	0	6	6
P	1	3	3	3
Q	2	2	0	0
R	1	1	4	2
S	5	2	2	7
T	9	2	7	0
Ave.	4.05	2.58	4.52	3.78
$P.E._m$	0.34	0.23	0.34	0.40
	$P.E._d$ 0.41		$P.E._d$ 0.52	

per cent of retention was 30.7, for the second 26.8, for the third 30.9, and for the fourth 27.6. In each case there is a lower percentage retained of the list learned in noise than of the list learned in quiet. Moreover adaptation does not seem to have helped as far as retention goes for the difference in percentage is about as great between lists three and four as between one and two.

We have so far seen that in spite of extra expenditure of effort and to some extent the aid of articulation to overcome the effect of the noise the amount of material learned and the amount retained after two days was less in the noisy conditions than in the quiet. Here we have a task that suffers when attention is divided in spite of the efforts of the subjects to overcome the disturbance. The increased effort likely helped them somewhat in the learning by increasing their immediate memory as is shown by the learning scores, but the retention scores show that it did not aid them to form any

permanent associative bonds, which is the essential thing in memorizing. This may not have been so had we used sense material, as increased activity and effort would in this case probably aid the subjects in getting additional bonds; but when the process is that of rote memory concentrated attention is more essential than increased effort.

Besides the effects already noted we have evidence that the range of attention was diminished during the noisy periods. The subjects were not asked to memorize the words used and the constant change of arrangement with each presentation discouraged any attempt at forming successive associations. To test memory for the words used would, under these conditions, be a test of memory for incidental factors in the experiment. To get a measure of the effect of the noises on these incidental associations both a recall and a recognition test were used. Two days after the experiment each subject was asked to write all the words he could recall that were used in the experiment, and after he had done this he was

TABLE VII

Word Recall. The Scores Indicate the Number of Words Recalled in a Possible Ten

Condition	Quiet	Noise	Quiet	Noise
Period	A	B	C	D
Subject *A*	0	0	1	1
B	2	0	1	1
C	2	2	1	1
D	3	2	1	3
E	3	1	1	4
F	4	6	2	5
G	1	1	1	1
H	1	0	1	1
I	2	2	4	4
J	1	1	0	0
Period	B	A	D	C
K	1	2	2	0
L	2	2	3	2
M	1	2	1	0
N	3	1	0	0
O	3	1	4	2
P	1	2	3	0
Q	0	1	0	0
R	2	2	4	1
S	3	2	2	2
T	1	1	1	0
Ave.	1.8	1.55	1.65	1.4
$P.E._m$	0.17	0.16	0.20	0.22
	$P.E._d$ 0.23		$P.E._d$ 0.30	

TABLE VIII

WORD RECOGNITION. THE SCORES INDICATE THE NUMBER OF WORDS CORRECTLY RECOGNIZED WHEN MIXED WITH AN EQUAL NUMBER OF WORDS THAT HAD NOT BEEN USED IN THE EXPERIMENT. THE HIGHEST POSSIBLE SCORE IS TEN

Condition	Quiet	Noise	Quiet	Noise
Period	A	B	C	D
Subject *A*	5	2	4	4
B	7	7	9	3
C	7	7	7	9
D	6	6	6	9
E	7	3	5	7
F	7	9	6	8
G	6	7	9	8
H	7	3	6	5
I	10	9	10	5
J	5	9	7	3
Period	B	A	D	C
K	8	8	10	9
L	4	5	6	7
M	7	8	9	9
N	4	3	7	3
O	5	2	7	4
P	5	4	4	5
Q	6	5	6	2
R	5	6	5	7
S	9	8	7	8
T	7	6	8	6
Ave	6.35	5.85	6.90	6.05
$P.E._m$	0.24	0.37	0.27	0.39
	$P.E._d$ 0.44		$P.E._d$ 0.47	

presented with a sheet containing all the words used mixed with forty new words and asked to check all those he recognized, in accordance with the directions given above. The scores for these two tests are given in Tables VII and VIII respectively.

In the recall test the average number of recalls is less for the noisy periods in each case than for the quiet, though owing to the limited number of recalls the difference is not very significant. In the comparison of the first two periods five subjects recalled a greater number and seven less, while for eight the scores in each condition were equal. A comparison of the second periods shows that three recall a greater number for the noisy period, seven a less number and ten the same.

In the recognition test the differences are a little more significant, the second comparison showing a difference 1.9

times the probable error of the difference. The differences in both these tests are not positive evidence of a very strong influence of the distractions on incidental memory, but that in each case the difference is in the direction of inferiority for the noisy period together with the fact that in every case but one the difference is greater than the probable error of the difference gives the evidence cumulative force.

IV. Conclusions

1. The learning of paired associates consisting of a three letter word and a digit is interfered with by noise, the interference being greater at first than later in the working period.

2. The time which the subjects take to respond in learning when they are given the privilege of setting their own pace is not changed to any reliable degree by noise. What little difference in time is apparent is in the direction of quicker reactions in the noisy conditions.

3. Articulation as indicated by the breathing ratio is used to overcome the effect of the noises more in the latter part of the working period than in the earlier part.

4. It is evident from the severity of the blows given in reacting that the subjects were in a state of greater tension in the noisy conditions than in the quiet. This tension was greater in the first noise period than in the second.

5. The amount of material retained after two days is less for material learned in noisy conditions than for that learned in quiet. The difference is as great for the second period of noise as for the first.

6. That the range of attention for incidental factors of the work in hand is decreased by noise is shown by the lower recall and recognition scores for the words of the lists used in the noisy periods.

7. The results obtained in this experiment show that the task of memorizing material which is from its nature rather bare of associative bonds is a good one to test the effect of distraction. While adaptation is apparent in the learning it does not counterbalance entirely the distraction effect, and the additional tests of retention and incidental associations give further data for interpretation. Material like word paired-associates might however show the reverse effect as the additional effort called forth by the more difficult situation might lead to the forming of more permanent associations.

8. Our results taken together indicate that irrelevant noises have a permanent effect upon an individual. One may adapt

himself insofar as the assigned task is concerned but if tests are taken for which he is not prepared it will appear that the adaptation is through loss in some other direction. He may show less permanency of associative bonds, he may have done the work at a greater strain, he may have used some trick to overcome the effect, or he may have grasped fewer of the incidental features of the work; he may show any or all of these effects and others also might be found had we adequate measures, but in some way he pays in order to keep his speed and accuracy up to par.

9. Finally, the value of a group of measurements simultaneously taken cannot we believe, be overestimated. If a dozen tests are given to the same individual at different times he will be able to adapt himself to each test in spite of unfavorable conditions and the pooled results of such a group of measures will show nothing. If however he is given one test and a dozen other measures are taken while he is working, if there is any effect of the conditions it will show itself in some way or other. Any one can adapt himself to conditions so that the work in hand will show little or no effect of any ordinary disturbance or even of an extraordinary one (it is the habit of the human organism to so adapt itself) but the object of studying the effects of environmental conditions is not to find whether one can so adapt himself, it is to see by what means he does so. Regardless of the difficulties involved in the way of technique the method of simultaneous measures must take precedence over the one-test-at-a-time method, the futility of which as a test of conditions has been fully demonstrated in recent years.

THE INTELLIGENCE QUOTIENT OF FRANCIS GALTON IN CHILDHOOD

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The writer does not remember to have seen Francis Galton classed among boy prodigies. Indeed, Galton's main contributions to science were given to the world at so late a date in his life that he is not infrequently mentioned as an illustration of late maturing genius. *Hereditary Genius* was published in the author's fiftieth year; *Natural Inheritance* in his sixty-eighth year; and on the practical side the most important work of his life was not accomplished until he was more than eighty years of age.

In the recently published Volume I of Karl Pearson's *Life, Letters and Labors of Galton,* there is ample evidence that Galton was a boy of unsual attainments and that he was extraordinarily precocious. The biography in question departs radically from the usual type of biography by presenting documentary evidence regarding the more important events in the life of its subject, and that concerning Galton's childhood is especially full and significant. From the evidence given, one is justified in concluding that between the ages of three and eight years, at least, Francis Galton must have had an intelligence quotient not far from 200; that is, his mental age at that time was not far from double his actual age.

The significance of this will be apparent when we say that after diligent search in several cities and several counties in California—a search including many thousand of children in scope—the highest intelligence quotient we have yet found is 170. The number that we have found going above 150 can be counted on the fingers of one hand.

From early childhood Galton was under the instruction of his sister, Adèle, herself a mere child. "She taught him his letters in play, and he could point to them all before he could speak. Adèle had a wonderful power of teaching and gaining attention without fatiguing. She taught herself Latin and Greek that she might teach him. She never had him learn by heart, but made him read his lesson, bit by bit, eight times over, when he could say it. He could repeat much of Scott's *Marmion* and understood it all by the time he was

five." (Quoted by Pearson from Elizabeth Anne Galton's *Reminiscences*).

Pearson further informs us that Francis knew his capital letters by twelve months and both his alphabets by eighteen months; that he could read a little book, *Cobwebs to Catch Flies,* when two and a half years old, and could sign his name before three years. The following letter has survived from his fourth year, a letter which has been endorsed by his mother, saying that Francis wrote and spelled it entirely himself:

"My
dear
Uncle
we have
got Ducks. I know
A Nest. I mean
to make a
Feast."

The day before his fifth birthday he wrote the following letter to his sister:

"My dear Adèle,

I am 4 years old and I can read any English book. I can say all the Latin Substantives and Adjectives and active verbs besides 52 lines of Latin poetry. I can cast up any sum in addition and can multiply by 2, 3, 4, 5, 6, 7, 8, [9], 10, [11].

I can also say the pence table. I read French a little and I know the clock.

Francis Galton,
Febuary 15, 1827."

The only misspelling is in the date. The numbers 9 and 11 are bracketed above, because little Francis, evidently feeling that he had claimed too much, had scratched out one of these numbers with a knife and pasted some paper over the other!

This document should have great interest for those who have worked with mental tests. That Francis at less than five years could read any English book demonstrates beyond any possible doubt that he was as far advanced at this time as the *average* English or American child at nine or ten years. It is an accomplishment which we do not believe is possible to a mental age of less than nine years with any amount of formal instruction. It is certain that our subject's accomplishments did not include merely the ability to pronounce words mechanically, for there is ample evidence from other sources that at this early age he read with understanding.

Again, at this age Francis had learned to do any sum in addition, and had learned all but the hardest part of the multi-

plication table. This indicates, at least, nine-year intelligence, for we have found that, however old a child and however much school instruction he may have had, the multiplication table is seldom mastered thoroughly much below the nine-year level. Further, his knowledge of the "pence table" indicates an acquaintance with the coins and their values such as children ordinarily do not have before something like eight years.

Besides informing us that Francis had, at this tender age, gotten quite a start in French and Latin, the above letter also tells us that he "knows the clock"; that is, presumably, he was able to tell the time of day by the clock. This performance has been definitely standardized at the mental age of 9 to 10 years[1], and it is almost never passed before the mental age of eight years.

The reader may raise the question whether it is safe to accept a child's own statements with regard to the above points. It would not be, of course, if there were no corroborative evidence. The fact that there is such evidence from many sources, and the fact that little Francis was known to be as remarkably conscientious as he was intelligent, justifies us in accepting the above statements without the slightest discount.

The fact that Francis' reading at the age of five years was intelligent and not of the mechanical kind, is demonstrated by his ability at that age to offer quotations which would fit a given situation. For example, when he was five years old, a boy friend asked his advice as to what he ought to say in a letter to his father, who, it seems, was in danger of being shot for some political affair. Little Francis replied immediately from Walter Scott:

> "And if I live to be a man,
> My father's death revenged shall be."

Again at the age of five, he was found holding a group of tormenting boys at arm's length, shouting meanwhile,

> "Come one, come all. This rock shall fly
> From its firm base, as soon as I."

By six, under the tutelage of Adèle, he had become thoroughly conversant with the Iliad and the Odyssey. At this age, a visitor at the Galton home made Francis weary by

[1] Gertrude Hall: Eleven Mental Tests Standardized. Bulletin of State Board of Charities, New York, 1915, pp. 79ff.

cross-questioning him about points in Homer. Finally, the boy replied, "Pray, Mr. Horner, look at the last line in the 12th book of the Odyssey" and then ran off. The line in question reads, "But why rehearse all this tale, for even yesterday I told it to thee and to thy noble wife in thy house; and it liketh me not twice to tell a plain told tale."

It seems that Adèle also taught Francis a good deal about entomology, and at six and seven years he was active and persistent in collecting insects and minerals, which he is said to have classified and studied in more than a childish way. It has been shown by Mrs. Burk[2] that collections of an analytical and classificatory type are not common before twelve or thirteen years. Here, again, we find evidence of an intelligence quotient not far from 200.

Pearson quotes the following letter written by a visitor at the Galton home on December 28, 1828:

" . . . . "The youngest child, Francis, is a prodigy. He is seven next February and reads 'Marmion', 'The Lady of the Lake', Cowper's, Pope's and Shakespeare's works for pleasure, and, by reading a page twice over, repeats it by heart. He writes a beautiful hand, is in long division, and has been twice through the Latin Grammar; all taught by Adèle."

At the age of eight, Francis was taken away from home to attend a boarding school. Here he was placed in a high class, although the boys in it ranged up to fifteen years. Since this was a private school attended by children of a superior social class, it is altogether likely that his fourteen and fifteen-year-old classmates were themselves above the average mental level of that age; hence Francis must by this time have reached a mental level of not far from that which is median for sixteen years.

In his first year at this school, we find Francis writing to his father in these words: "I am very glad that you have left off being a banker, for you will have more time to yourself and better health." This little quotation certainly betokens a degree of filial solicitude by no means common to children of this age. Such altruism does not ordinarily develop so early. The words fit sixteen-year much better than eight-year intelligence.

Francis' interests at the age of ten are indicated by the following letter:

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[2] Caroline Frear Burk: The collecting instinct. *Pedagogical Seminary,* Vol. VII, 1900, pp. 179-207.

December 30, 1832.

My dearest Papa:

It is now my pleasure to disclose the most ardent wishes of my heart, which are to extract out of my boundless wealth in compound, money sufficient to make this addition to my unequaled library.

The Hebrew Commonwealth by John.........	9
A Pastor Advice............................	2
Hornne's commentaries on the Psalms........	4
Paley's Evidence on Christianity..............	2
Jones Biblical Cyclopedia......................	10
	27

It is hardly necessary to comment on the above letter as an indication of the boy's mental maturity. It speaks for itself.

Francis' interests, however, were not wholly literary, for at the age of thirteen he gave us "Francis Galton's Aerostatic Project." It seems this was a series of drawings representing a flying machine. It was to work by large, flapping wings with a sort of revolving steam engine, and was supposed to carry five passengers, a pilot and an engineer.

At the age of fifteen, we find the youth Galton expressing in his letters to his father serious opinions on mind training, the relative value of classics and English, and other matters of educational theory. These opinions were voiced by him again some sixty years later, substantially without change.

At the age of fifteen Francis was admitted to the general hospital at Birmingham as a medical student.

It is well known that, in general, a high correlation obtains between favorable mental traits of all kinds; that, for example, children superior in intelligence also tend to be superior in moral qualities. Francis Galton was no exception to this rule, as indicated by the following letter written by his mother when the boy was only eight years old: "Francis from his earliest age showed highly honorable feelings. His temper, although hasty, brought no resentment, and his little irritations were soon calmed. His open-minded disposition, with great good nature and kindness to those boys younger than himself, made him beloved by all his school fellows. He was very affectionate and even sentimental in his manners. His activity of body could only be equalled by the activity of his mind. He was a boy never known to be idle. His habit was always to be doing something. He showed no vanity at his superiority over other boys, but said it was a shame that their education should have been so neglected."

After Mr. Pearson has given us all the above significant information, it is astonishing to find him commenting upon it as follows: "The letters we have quoted from these early years may appear to the reader to contain little of note. They are, indeed, just what a healthy normal child would write, but it is that very fact that makes them essential human documents and gives them their fundamental interest. . . . . Need we attempt to see signs of exceptional ability or to discover foreshadowings of future achievement in these outpourings of healthy childhood? I do not think we can say more than that Francis Galton was a normal child with rather more than average ability", etc.

Mr. Pearson's error is of a kind which is now coming to be generally recognized by those who work with mental tests; that is, an error due to the failure to take into account the significance of a mental performance *in terms of the mental age to which it corresponds.* Pearson did not know, and the average teacher does not fully appreciate, that a child of four years who is able to do the things characteristic of a child of seven or eight years is a genius of the first order. It is hard to get people to understand that what a child is able to do has no significance unless we take age into account.

The opposite error is no less common; that is, for a mentally retarded child in a grade far below his age to be considered perfectly normal and average in intelligence. Only recently we were consulted by a teacher regarding a child who was described as "slow to learn". The child in question was twelve years old and in the first grade, and we suggested to the teacher that in all probability the child was feeble-minded. We were met, however, with the most positive assurance that the little girl in question could not possibly be feeble-minded, that she was actually learning the work of the first grade, and that her normal mentality was shown by her motherly interest in her little six-year-old classmates. Without arguing the matter further, we urged the teacher to bring the child for a Binet test, with the result that she was found to have a mental age of a little less than 6 years by the Stanford Revision. This child had been in school several years and had had every opportunity to learn, except the advantage of endowment. Experience has taught us that such a subject will never reach the mental level of seven years, however long she may live.

This teacher's error may seem to some almost incredible. In reality it is an error of about the same degree as that made by Mr. Pearson, though in the opposite direction. Similar errors, though perhaps not quite as great, are abundant even

in the writings of psychologists on mental tests. They are to be found over and over, for example, in Professor Holmes' recent book "The Backward Child."

Studies are now in progress at Stanford University on exceptionally intelligent children, and we should especially like to receive information about children who test much above 150 by the Stanford-Binet scale. As already stated, the highest intelligence quotient that we have found is 170. We need accurate case descriptions and follow-up work on cases testing 150 to 200.

PSYCHOLOGY AND SOCIAL SCIENCE

By H. W. Chase, University of North Carolina

Much has been written in the last two years of the necessity of mobilization for an organized attack on scientific and industrial problems. Natural scientist and economist are being pressed into service as never before. But there seems little recognition of the fact that industrial and economic problems are but fragments of the whole problem of human life; that, important as their solution is, it is but a means to an end of improving the general social relationships of man with man. As Ellwood[1] has said, there is really only one social problem, that of "the relationships of men to one another." Problems of labor and the rest are but phases that can never be satisfactorily treated so long as they are seen as isolated. Western civilization, that had been welded by the advances of natural science into what Wallas has called "the Great Society," failed throughout the whole of the era which has now closed to recognize this fact. Its record of social failures is as impressive as its record of scientific success. Of the social sciences, economics alone has impressed itself on the "men of light and leading" to whom its destinies have been committed. With most men of affairs, it still seems an accepted doctrine that the remedies for all social ills are economic. The "economic man," though officially as dead as the faculty psychology, is still in reality just about as much alive. The idea that the advances of natural science would automatically bring about a solution of all social problems has for some time ceased to appeal to the more intelligent of men; but in advancing beyond it, the prevalence of economic interpretation has resulted in a social attitude which is both one-sided and dangerous.

There have not, indeed, been wanting of late years signs of increasing realization in many quarters that social problems must be more widely conceived if they were to be adequately understood—and that the task of understanding was yearly becoming one of more pressing moment.

"Throughout the politics and literature of the twentieth century," wrote Graham Wallas in 1914, "one traces this

[1] Ellwood, C. A. The Social Problem. N. Y., Macmillan, 1915.

fear, conscious or half-conscious, lest the civilization which we have adopted so rapidly and with so little forethought may prove unable to secure either a harmonious life for its members or even its own stability. The old delight in the 'manifest finger of destiny' and 'the tide of progress,' even the newer belief in the effortless 'evolution' of social institutions are gone. We are afraid of the blind forces to which we used so willingly to surrender ourselves. We feel that we must reconsider the basis of our organized life because, without reconsideration, we have no chance of controlling it. And so behind the momentary ingenuities and party phrases of our statesmen we can detect the straining effort to comprehend while there is yet time. Our philosophers are toiling to refashion for the purposes of social life the systems they used so confidently to offer guidance for individual conduct. Our poets and playwrights and novelists are revolutionizing their art in the attempt to bring the essential facts of the Great Society within its range."[2]

Social phenomena can be controlled only as they are understood, not by hasty and sentimental attacks upon them. There is no place in the social programs of the future for Mr. Wells' "Gawdsaker." Organized attempts at control there must be, if national rivalry and class discontent are not to be more bitter in the future than ever before; but such attempts must be broadly conceived and must base on a knowledge of the facts about the behavior of men in their social relationships. Every scrap of available knowledge of human behavior that bears on men's relations with each other must be utilized. Here is the greatest task and the greatest opportunity of applied psychology. Psychology needs to put itself to work not only as a series of isolated techniques but as an instrument for the understanding and control of social conduct.

The relations which have existed between psychology and the social sciences have been for the most part quite unsatisfactory. The majority of students of social phenomena, when they begin to psychologize, do so in terms that seem to the psychologist both naïve and antiquated. It is perhaps hardly fair to single out among so much bad psychology one case for illustrative purposes, but the point will be the better for concreteness. A text on sociology published within the last year devotes a chapter to the subject of "social laws."[3]

[2] Wallas, Graham. The Great Society. N. Y., Macmillan, 1914, p. 14.

[3] Blackmar, F. W., and Gillin, J. L. Outlines of Sociology. N. Y., Macmillan, 1915.

After a preliminary warning to the effect that social laws are less exact than those of physics, " for, since human beings possess thought and will, their actions are not so definitely determined as those of atoms," the authors state and discuss some eighteen laws of social action formulated by various writers. The collection is to the psychologist an interesting one. He is met at the outset by the law that " each individual seeks the largest return for the least sacrifice." The explanation follows that " whether we consider wealth getting or wealth using, religion or art, culture or learning, or, indeed, life in any of its various important phases, the individual is seeking his highest good or best interests so far as his powers or capacities will permit." The man who always seeks " his highest good or his best interests " is a creature more familiar to the economics of the nineteenth century than to the psychology of the twentieth. Sundry chapters on habit, instinct, and emotion have disposed of him in psychology, but, it seems, not in sociology. The " law " is only true when it is taken in a sense quite other than that of the authors—let us say, with Thorndike, that the individual seeks satisfying situations and avoids annoying situations, and that there is no necessary correlation between such situations and " highest good," or " best interests." And, even then, the problem is simply stated—Thorndike has devoted a volume to a preliminary attempt at the outlines of its solution.

Again, we meet the law that " the greatest good to the greatest number, or social well-being, is the aim of social action." This may well be true, but in what other department of knowledge claiming to be scientific is a statement of purpose regarded as a law? Throughout the whole section, the laws which are cited are for the most part mere unanalyzed statements, often so vague as to be meaningless. Consider, for example, the law that " in the absence of interferences, imitation spreads in geometrical progression." Psychology has had a deal to say of imitation in the last few years—Tarde seems, however, not yet fully disposed of.

Consider again these two laws: " Each individual has a schedule of choices ranging from the most desirable objects to the least desirable " and " Individual minds respond similarly to the same or like stimuli." Immediately afterward comes this statement regarding the latter law: " It must not be carried too much into detail, or it will conflict with the one previously stated " (the individual schedule of choices). It seems impossible to interpret the two laws together in any other way than by some such statement as that " people value

things differently, so that they don't always behave in the same fashion, but sometimes they do." The latter law, we are further told, is "a well-established principle."

But by this time one is willing to admit all the authors say—and more—with regard to the inexactness of such social laws. What McDougall wrote a few years ago of the attitude of students of the social sciences will bear repetition—"some do lip service to psychology, but in practice ignore it, and will sit down to write a treatise on morals or economics, or any other of the social sciences, cheerfully confessing that they know nothing of psychology. A certain number, perhaps the majority, of recent writers on social topics are content to take as their psychological foundation the vague and extremely misleading psychology embodied in common speech, with the addition of a few hasty assumptions about the mind made to suit their particular purposes."[4]

The realization that psychology should play more than a subordinate rôle in social science is, indeed, surprisingly recent. It was as late as 1908 that Ross could excuse his trepidation in putting out his book on the ground that it was "the first treatise, in any language, professing to deal systematically with the subject of social psychology."[5] As for the psychologists *von Fach* who have made extended contributions to social interpretation, they can be counted on the fingers of a hand.

Much of what the social sciences have been doing is, indeed, scientific only in name. Instead of beginning with the study of human behavior in its social aspects and proceeding to utilize the facts thus derived to further understanding, the social sciences have too often attempted to arrive at their conclusions by adopting at the start a set of theories as to what society was about, and then amassing facts to show that the social process was really what, on other grounds, they were already convinced that it ought to be.

It has been said of Spencer, for example, that "he was already charged with political preconceptions when he approached science, and he sought to find in science examples or analogies to point a moral already drawn and adorn a tale whose plot was already sketched."[6] To Spencer, indeed, the scientific treatment of social problems meant, not so much

[4] McDougall, Wm. An introduction to Social Psychology. Boston, 1909, 2d edition.

[5] Ross, G. A. Social Psychology. N. Y., Macmillan, 1908.

[6] Barker, Ernest. Political Thought from Herbert Spencer to the Present Day. Holt, New York, 1915, p. 85.

their treatment by scientific method, as the application to human society of the facts and theories of biology. It was an extension of the content, not the method, of natural science to human affairs, that engaged his attention.

Spencer is not the only student of social science to fall under the spell of the biological analogy. But even those who have been most convinced of the correctness of such an approach are far from satisfied with the results which it has so far yielded. Just at present, this dissatisfaction expresses itself by a more insistent effort to get at the correct biological premises, to abandon Spencer in favor of Darwin. Two books, published in the last year or two, may serve to illustrate what is happening.

The author of the first of the two[7] begins by expressing the conviction that the biological interpretation of society rests not on an analogy, but on a real identity. The difficulty is that our social thinking has been dominated by the philosophy of Spencer instead of the science of Darwin; let us therefore go to the real facts. But, he goes on to say, we must not go to the "Descent of Man," in which Darwin has sketched his own social applications, for in these chapters Darwin has for some reason "been led to assay waters beyond his depth." He continues: "This part of the 'Descent' had better have been left unwritten, for, in default of the usual mountains of data from which he was wont to draw his weighty inductions the great scientist was led to wander hopelessly among the unfamiliar and unfathomable quicksands of the metaphysical and intuitional. In so doing he presents but a sorry spectacle to his admirers."

The author of the most recent of the biological attempts[8] also deplores the influence of the metaphysics of Comte and Spencer. He too, will return to Darwin and re-examine the facts. Above all, let us go to the "Descent," for there we find, sketched by Darwin himself, a theory of social progress which furnishes "the clear guiding principle which seems to offer for the social sciences, something of the vitalizing organization and system which the discovery of the Newtonian law of gravitation gave to the physical sciences in the seventeeth century."

It is needless to say that the two men come to quite different conclusions. The biological analogy, indeed, may lead to as many systems as there are human temperaments on which to

[7] Keller, A. G. Societal Evolution. New York, Macmillan, 1915.

[8] Nasmyth, George. Social Progress and the Darwinian Theory. N. Y., 1916.

base them. The method is not that of science, but of speculation.

This naturally does not mean that biology has nothing to do with social affairs. It does mean that the assistance rendered by biology to the study of social phenomena cannot take the form of setting up an *a priori* set of categories into which the facts must be fitted.

Those who have looked to economics for an explanation of social phenomena of all sorts have been guilty of a somewhat similar error. "It is probably safe to say," writes Carver, "that the economist is the only one of the various students of society who has accomplished much in the way of perfecting his analysis. On the purely economic side of social life considerable progress has been made in this direction, and it therefore seems probable that the method of sociology will be an expansion of the method of economics. However, the chief danger is that if sociology is to be developed from the economic standpoint, and by an expansion of the method of economics, the purely economic factors will be overemphasized."[9]

It would seem, however, that just as in the case of biology, it is not so much an extension of the method of economics as of its content, to the field of society, that is to be feared. For a variety of reasons, the economist has been able to make use of the methods of science in general in his own field more than have students of the related disciplines. To say that the method of economics may prevail in the social sciences is just to say that they will become more scientific; to say that its content may shape social thinking as a whole, on the other hand, means that the whole is seen in terms of one of its parts. It is in the latter sense that economics has influenced many thinkers—Marx and the adherents of the doctrine of economic determinism generally are of course classical examples.

Many economists, far from insisting that their formulations offer all-sufficing explanations, have seen clearly enough that, even within the territory of their science itself, problems arise that cannot be settled in solely economic terms. It is sufficient to recall the vexed question of value. A growth of scientific habits of thought in this discipline that might well be emulated by others of the social sciences is evidenced when one compares the broad lines of treatment of this particular problem by, for example, the modern Austrian economists with the mid-century dogmatism of John Stuart Mill.

[9] Carver, T. N. Sociology and Social Progress. Boston, Ginn, 1905, pp. 2-3.

"Happily," he wrote, "there is nothing in the laws of value which remains for the present or any future writer to clear up; the theory of the subject is complete: the only difficulty to be overcome is that of so stating it as to solve by anticipation the chief perplexities which occur in applying it; and to do this, some minuteness of exposition, and considerable demands on the patience of the reader, are unavoidable."

This propensity to explain all social phenomena by utilizing the data of some one science has expressed itself in still other forms. The title of Bagehot's "Physics and Politics" is sufficient indication of still another tendency, while others have followed in the footsteps of Buckle, and found their talisman even more than he in geographic influences.

Least scientific of all, perhaps, are those views which find the key to the understanding of social processes in the concept of society as an organism. Since the time of Hobbes, many thinkers have succumbed to the attractive analogy; some stating its implications in metaphysical terms, others, like Spencer, coming to the same result from their biological bias. How far such doctrines may go is shown by a writer like Lilienfeld, who begins by asserting that society is not merely like, it *is*, an organism. Its cells are the nervous systems of individual men, its tissues their class groupings. Environment furnishes the "social intercellular substance," government is the brain, while the superiority of the social organism lies in the fact that all its cells are nerve cells. Society, like other organisms, grows old, suffers disease and death.[10]

German social thought, with its distinction between the organized state and society, holds largely, so far as the former is concerned, to some form of the organic analogy; while that group of writers who, like LeBon, hold that even the temporary forms of human association bring into being some sort of over-consciousness, different as their conceptions may be in other respects, agree in attributing to society a sort of super-individuality in a fashion that recalls the scholastic realist.

What has been said in the preceding paragraphs is not in any sense to be regarded as an attempt to sketch the main tendencies in social thought; it is merely a series of illustra-

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[10] Von Lilienfeld, Paul. Gedanken über die Socialwissenschaft der Zukunft. 1873-1881. 5 vols. For a summary of the whole organic school, see Coker, F. W. Organismic Theories of the State. N. Y., Columbia University Studies in History, Economics, and Public Laws, Vol. 38, No. 2, 1910. The sketch of Lilienfeld's views above is summarized from this monograph.

tions of the fact that preconceived systems, one-sided theories, schools of thought, have tended in social science to take the place of an unbiased study of phenomena. The student of social problems, as has been said above, has too often failed to make the distinction between appropriating the method of science and appropriating its data. He has forgotten that, while the method of science is the same in all departments, the data of particular sciences cannot forthwith be transferred to the problems that arise in other fields. Not all students of society, to be sure, have been so wanting; especially of late years there has been a growing realization of the inadequacy of such methods of attack. And the psychologist may well remember that it is not so long ago that psychology, with the theories of the association school, was dominated by the same tendency toward direct application of the subject-matter of an alien science to its own domain. The attempt to apply the content of mechanics to psychology failed, just as corresponding attempts in the social sciences have failed.

The difficulty which the social sciences have encountered with their method is one which meets the student of human behavior in any field. Natural science has succeeded after three centuries of effort in establishing in its own field a particular method of viewing natural phenomena. But so foreign to the mind of the race are scientific habits of thought, that their extension to human nature still meets with active resistance.

The scientific viewpoint means, fundamentally, two things. It means that the order of phenomena dealt with must be regarded as uniform, and that facts and those statements of uniformity which make up the laws of science are discoverable only by properly controlled methods of observation, comparison and experiment.

The belief that the order of nature is uniform means to the scientist that the march of material events is not upset by the capricious irruption of external immaterial agencies. The opposite belief, we have come to see, is the essence of the older world-view of animism.

Many social writers are frankly animists; they regard social phenomena as incapable of exact analysis just because they are to some extent capricious. Pleasure, or pain, or reason, forces quite outside the material chain of events, are conceived as entering somehow to modify their nature. Some are indeed quite willing to admit that the activities of a society as a whole are uniform, if only the group is large enough, but seem to speak as though this uniformity were the result

of the mutual cancellation of many capricious individual activities. Writers on ethics still double and twist to assure on the one hand a fair amount of determinism and to save, on the other, the "freedom of the will." Legal doctrines of responsibility still rest on views of human nature that are nothing short of archaic.

The psychologist has cast off the cruder forms of animism, but even here its more subtle forms are still prevalent to a surprising degree. For to suppose that a nervous system which is a material mechanism developed by natural forces, is at times set in activity by psychic forces of any sort, which interrupt the orderly course of material phenomena, is to assert the very view against which science has been forced to struggle in one field after another. Yet such a defense of animism as McDougall has given us is not an isolated phenomenon.

In Ladd and Woodworth's "Elements of Physiological Psychology," the point of view is frankly animistic, and that not only in the final section of the book. In the midst of a discussion on "The Nervous Mechanism," for example, we are told that "The molecules of its central organs are capable of assuming inconceivably varied relations to each other, of thus transmitting and redistributing the nerve commotions which reach them along the incoming tracts, and even (it would seem) *of starting automatically outgoing disturbances in response to self-conscious sensations and ideas.*"[11] In his recently published text, Pillsbury too comes out frankly for animism, only cautioning that for the avoidance of vagueness it is best to "assume as little interaction between the series as is possible."[12]

The functionalist has been in this regard the chief of sinners. Watson has said of him that he "actually thinks in terms of interaction and resorts to parallelism only when forced to give expression to his views."[13] Many of his statements seem indeed capable of no other interpretation.

Such a psychology is exactly in the position in which physics was put by the famous statement that water rises in a tube from which the air has been exhausted because "nature abhors a vacuum." Such a physics believed firmly enough that the simpler cases of the behavior of water could be dealt with by scientific methods, but that when the cir-

[11] P. 283. Italics mine.

[12] Pillsbury, W. B. Fundamentals of Psychology. N. Y., Macmillan, 1916, pp. 92-97.

[13] Watson, J. B. Psychology and Behavior. Psy. Rev. 20, 1913, 158.

cumstances became more complicated, it was necessary to suppose the intervention of a Nature-spirit whose whim it was to interfere with the uniformity of natural events because of its abhorrence of the state of affairs that would otherwise be produced. Science for the simple, animism for the complex; such in a word is the meaning of this explanation.

In precisely the same fashion, we make a distinction between the simpler and the more complicated cases of human behavior. We speak of reflexes in terms of muscles that contract and glands that secrete because a nervous system of a definite character has been excited by such and such external stimuli. The whole chain of events is uniform, it is matter for scientific investigation, it is made up of material factors which culminate in the functioning of the material structures of muscle and gland. But complicate the issue and we fly, as did physics, to the arms of animism. It is "idea" and "impulse" that determine the material changes in muscle and gland,—a spirit called "anger" that makes us flush and grow pale.

So long as the psychological problems which one sets himself have little to do with every-day human affairs, such an insistence on adherence to scientific doctrine throughout may seem of merely academic importance. But for one who is interested in putting psychology to work, the question as to whether he shall proceed as a scientist or as an animist becomes of large significance. For the easy assumption that psychic forces do function serves to stop investigation at the points where it most needs to be pressed.

One illustration will suffice. McDougall says, in effect, that meaning has no physiological explanation. There is no need to attempt to understand what complicated neural commotions may underly it—there are none. Such doctrine means that the psychiatrist, for example, who is interested in discovering and altering the "setting" of a patient's ideas, need never hope to hit upon any neural basis for his work. Therefore let him turn to vague notions of subliminal forces, subconscious selves, which end only in confusion. Psychology interpreted in such fashion only confounds the chaos that has so long held conceptions of the abnormal, and from which it is just beginning here and there to emerge.[14]

It is hardly to be expected that social science should have shown more respect for the uniformity of behavior in the group than has psychology in the individual. But the failure

[14] See in this connection Watson, J. B. Behavior and the Concept of Mental Diseases. Jour. of Phil. Psy., etc., 13, 1916, 589.

to analyse successfully social phenomena which are regarded as capricious contrasts sharply with the success of those sciences which have freed themselves throughout from animistic ideas. It would seem time that another method was attempted in the social analysis; and it can hardly hope for acceptance in the social field while the spirit of animism still rules in psychology.

Among the psychologists, it is those who represent the movement toward behaviorism who seem most clearly to have grasped the importance of a thorough-going scientific attitude. It is, accordingly, to this quarter that one looks most confidently in considering the future relations between psychology and the social sciences, and this not alone because the behaviorist is stressing the uniformity of behavior; the shift of emphasis for which he stands is such as to give the social sciences a psychological basis of a character which they have hitherto lacked.

So long as psychology thought of itself as the science of consciousness, it found itself in a curious dilemma which has many times been described. For either it must, if it were to be consistent, adhere to the scientific viewpoint, and refuse to let the very mental states which it set itself to describe play any part in human affairs, or it must lapse into animism. It is not a matter of chance that just those psychologists who have been most interested in putting their science to work have been, for the most part, those who leaned most strongly toward animism. Fortunately, psychology has not taken its self-imposed limitations too seriously, though as long as consciousness held the center of the stage, too many psychologists failed to be interested in anything that fell outside the domain of possible introspection.

The movement toward behavior, on the other hand, has not been devoid of extravagance. Those who, with Watson, hold that conscious states should be disregarded altogether seem to overestimate the possibility of getting an adequate picture of the more complicated sorts of cortical functioning through the study of the muscle-gland responses which are connected with them. McComas, in a recent article,[15] has shown convincingly the difficulty of supposing that each sort of stimulus arouses muscle-gland responses in patterns different from those resulting from any other sort of stimulus. It seems probable that the only adequate witness to much complicated functioning will always be found to be some form of intro-

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[15] McComas, H. C. Extravagance in the Motor Theories of Consciousness. Psy. Rev. 23, 1916, 397.

spection; that conscious states must always be studied, though, not indeed, as ends in themselves.

The essence of the behavior movement, shorn of such extreme views, seems rather to be the statement of psychology in terms of situation and response. The organism is always doing something, in response to situations that involve both the environment and its own physiological states, as depending both on its original "action system" and its acquired modifications. What it does is, so far as psychology is concerned, and in the higher types of organism, a matter of the passage of impulses to effector organs, and also to those portions of the nervous system which, when stimulated, respond by the appearance of mental states of various forms. The mental state is as real a form of response as is the change in effectors—the probability is, as already mentioned, that the type of response that involves the cortex and the consequent appearance of the mental state need not always be accompanied by, or at least not be adequately portrayed by, the effector changes.

But whether the response is accompanied by the appearance of a mental state or not is, to the behaviorist, incidental. He is interested in the response as response, not because it is or is not accompanied by consciousness. If consciousness appears, it is certainly the task of psychology to incorporate its description in any complete account of the response, and to make that description as adequate as possible. But if the conscious state does not appear, the study of the response is none the less not only "of interest to psychology," it *is* psychology. Introspection is thus one method, but not *the* method, of psychological investigation.

The advantages of such a position are many; not least is the fact that behavior, so concerned, holds out hope of a synthesis acceptable to all the jarring psychological sects—save perhaps those made up of philosophers in disguise. Genetic, abnormal, applied psychology, doctrines of the subconscious, the Freudian mechanisms, would all seem capable of statement in its terms.

That the student of social affairs will find it more to his liking and profit than the traditional views seems sure. Indeed, such a psychology seems to hold out the only hope of a scientific social analysis. Sumner's stimulating conception of the "mores," for example, is easily capable of restatement in its terms; questions as to the part played by "reason" in society take on a quite different character when one comes to think of reasoning as a sort of response involving a high degree of analysis of a problem-situation. Instead of vague

discussion of pleasure and pain as social forces, conceptions of satisfying and annoying situations such as Thorndike has reached, with the many lines of investigation which they open up; instead of concepts of instinct and emotion as terms which explain social behavior, the realization that they are but convenient classifications of various types of response. Such illustrations, chosen at random, may serve to show something of the advantages to social science of a psychology of behavior, a psychology which is both scientific and usable.

The argument of the preceding pages is, in brief recapitulation, that the social sciences have often failed to distinguish between the method and the content of science, with the result that the term "social science," as used by many writers, is a misnomer. To be scientific, students of social problems need to base their work not on the laws of biology or economics, but on the laws of human behavior. And, further, human behavior throughout its whole extent must be conceived in scientific terms; as uniform and as discoverable only by adequately controlled investigation. That the relations between the social sciences and psychology which furnishes the material for the study of behavior, have not been closer, is deplorable, but psychology has often failed to be usable when it was scientific, or scientific when it was usable. The social sciences can, however, look with more confidence to a psychology that states its problems in terms of situation and response than to that which was occupied too exclusively with consciousness.

It remains to be seen whether the more intricate sorts of social phenomena are too complex to yield to control even after considerable progress in their scientific analysis is made. But one who considers the immense increase in control over natural phenomena which has resulted from the analyses of natural science can but take an optimistic attitude toward this most important of all problems. Just as in natural science it is the attempt to understand nature apart from immediate practical considerations which has resulted in control almost as a by-product, it would seem that the way to "apply" psychology to the social field is not primarily to set it to work to devise immature methods of immediate social control, but to utilize it as an instrument which shall make possible a more complete social analysis, and trust that, as in the natural sciences, control will follow inevitably in its own good time. This is not doctrine acceptable to the reformer in haste, but it is consistent with the whole history of science. Even a generation of work by scientific methods may bring our analysis to a state from which a surprising increase in control will follow.

PSYCHOLOGY OF THE BLIND*

By C. F. Fraser, K.B., LL.D.

Psychology is a science conversant with the states of the mind. It is the science of the conscious and subconscious mental life. Through the impressions of our bodily senses we gather experiences. These experiences are mentally realized, analyzed, compared and classified, and thus experiences are ripened into perceptions, perceptions into knowledge and knowledge into thought.

The science of psychological experience is confined to the individual. It is unshared experience, whereas in the physical sciences we deal with experiences common to the world and shared by everyone alike. Psychology deals with the object as I see it, with the sound as I hear it, with the odor as I smell it, with the hunger that I feel and with the emotions which I experience.

For centuries the philosophers of the world have been endeavoring to establish the relation of mind to body, but while distinct progress has been made, it has remained for psychology, the youngest daughter of science, to express the relationship in scientific terms and to open up a new and broad field for scientific investigation.

In the early days of my work in this school my interest in the mental conceptions of the blind was aroused by the discovery that the power to visualize objects perfectly was limited to those pupils who had retained perfect sight until they were from eight to ten years of age, while others who had lost their sight between four and eight years of age visualized imperfectly. Others again who were born blind, or who became blind shortly after birth, entirely lacked the power of visualization. I made many experiments to prove the correctness of my conclusions. At a later date my interest in the psychology of the blind was greatly quickened by association with one who had made a special study of the subject. I refer to Dr. Alexander Fraser, at one time a medical stu-

* Read before the American Association of Instructors of the Blind, at the Convention held at Halifax, Nova Scotia, July, 1916. This article has additional interest and authoritativeness from the fact that the author is himself a blind man.

dent in the University of Dalhousie, Halifax, N. S., and now Professor of Pathology, New York University & B. V. H. Medical College, and Assistant Pathologist to St. Vincent's Hospital. Together we tested eighty of the pupils of the school. Fourteen of these had lost their sight at eight years of age and upwards, thirty had become entirely blind between four and eight years of age, while thirty-six were born blind or became blind before four years of age. Placing a cube upon the table we asked each pupil to state how many sides of the cube he could see at once. Sixteen of them declared that they could see three sides, twenty-eight were certain that they could see four or five sides at a time, three of them could see but one side at a time, while thirty-three could see all six sides of the cube. Further experiments confirmed the first tests. The pupils were for the most part between ten and fifteen years of age. Those in the first class, namely those pupils who had lost their sight at eight years of age and upwards, visualized correctly seeing three sides of the cube; those in the second class, namely, those who lost their sight after reaching four years of age, visualized imperfectly, seeing four or five sides of the cube at once; those who were born blind or became blind shortly after birth lacked true visualization, thirty-three of them affirming that they saw all six sides of the cube at one time, while three of them saw but one side.

Pondering the results of the test with the congenital blind I was forced to the conclusion that they did not visualize in the sense that people with sight used this word and therefore that they could have no visual sense of form. In order to determine whether what they called "Seeing six sides at once" was a true "Seeing at once"—i. e., a synchronous grasping of the six sides, further tests were made. In answer to my question as to which side of the cube they saw first they all agreed that it was the upper face. There was not the same unanimity as to the front, back, right and left faces of the cube but again they all agreed that the lower face or that on the table was the last face they saw. The language is the language of persons with sight, which all blind persons naturally use, but the psychic event taking place in the minds of these young people was simply a rapid succession of tactual images realized psychologically as a cube. Further tests were made with the congenital blind as to their conception of other objects. For example, the pupils were asked what they thought of first in thinking of an armchair. The majority of those tested replied that they first thought of the right

arm of the chair, then of the left, then of the seat, then of the back and finally of the legs. A few thought of the left arm first, then the right, seat, back and legs. A still smaller number first thought of the seat, then of the right and left arms, back and legs. The greater number follow in imagination the order of perception for the right-handed, viz.—right arm, left, seat, etc. It is quite possible that those who thought of seat first serve to bring about an interesting point. It would be natural to suppose that the "order" in imagination is determined by mere "repetition" of the perception, but I think this is not so. The "order" is determined rather by "interest." The most interesting thing for the majority is "the finding" of the seat—hence the order of actual perception is determined. To the few the interesting thing is the "support" and the order of perception, which undoubtedly was the same for all, is neglected. These tests with many others proved that the mental conception of the congenital blind as to the appearance of objects differs from those of persons with sight, the difference being the result of the impressions made upon the mind by sight and by touch respectively. Sight is a comprehensive sense and through it persons who see receive a synchronous impression of the objects at which they are looking. They first see the object as a whole and afterwards take in the details. The sense of touch is limited to what comes in contact with the finger or other part of the body, hence the impression conveyed to the mind is made up of many individual sensations, or touches, which mentally give to the blind their conception of the appearance of the object touched.

The organ of touch is the skin of the whole body, including the membranes which line the mouth, the nostrils and other internal organs. Under the true skin are minute elevations, and investigation proves that these elevations are developed to their greatest degree in those parts of the body where the touch is most sensitive. Professor Webber, a distinguished German physiologist, instituted experiments to test the degrees of acuteness of touch in different parts of the body. Dr. Webber's tests have since been confirmed most fully. The tests prove that the sense of touch is most acute on the tip of the tongue and the tips of the forefingers. Blind women may frequently be seen threading their needles with the tongue. John Gough, the distinguished blind botanist, used to examine all unfamiliar plants with the tip of his tongue, although the tips of his fingers were as a rule sufficient for all practical purposes. In his old age a rare plant was placed in his hands.

After examining it he gave its name, remarking at the same time that he had only seen one specimen of the plant before, and that was fifty years ago.

Touch impressions with the blind must be actual impressions, or as one writer puts it, " With the blind actuality is tactuality."

Democritus, the ancient philosopher, was wont to say that all the senses were more or less modifications of the sense of touch. The truth of this saying is better understood when we realize that our special senses receive their impressions from the impact upon the organs of external forces. It is for this reason that touch is spoken of as the primary or mother sense.

In 1902 Dr. Griesbach, President of the German Association of School Hygiene, made a series of tests in order to compare the strength of the sense of touch in seeing and blind persons of the same age, and also to compare the touch impressions made upon the third, fourth and fifth fingers of the blind with those of the first fingers. For these experiments he used a pair of compasses with delicate rounded points, and much to his own surprise he found that the impressions made by the two points of the compasses could be distinguished in the second, third and fourth fingers of the blind at a shorter distance than the two points could be realized by the forefinger. In other words if the compasses were a line apart their points would impress themselves upon the forefinger of a blind person as one point, whereas the impressions made upon the second, third and fourth fingers would be those of two points.

Dr. Griesbach made many hundreds of experiments as to the comparative delicacy of touch in persons with and without sight. At the same time he tested the senses of hearing and smell. He sums up the results of his investigations in the following paragraphs which are herewith quoted in full:

1. " In the faculty of distinguishing impressions produced by touch there is in general no essential difference as regards the time free from labor between the blind and seeing. Small differences speak in favor of the seeing.

2. " In persons blind from birth the acuteness of the sense of touch is somewhat less than in seeing persons. In a few cases the rest of the sensorium also suffers in persons blind from birth.

3. " The blind have a less acute sense of touch in the tip ends of the forefingers than the seeing; and in the blind there

is a difference between the two forefingers as regards the faculty of receiving impressions.

4. "The blind need, especially as regards the hands, a stronger impression than the seeing if a distinct impression of touch is to be produced.

5. "In the faculty of locating the direction of sound there is no difference between the blind and the seeing.

6. "The ability to locate the direction of a sound varies in the blind as much as in the seeing and in both is to a very great degree shaped by the individuality of each person.

7. "As a general rule the direction of a sound is determined by the blind and seeing more accurately by hearing with both ears than only with one ear.

8. "There is no difference between the blind and seeing as regards the distance at which a sound can be heard and located.

9. "There is no relation between the distance at which sounds can be distinguished, and the ability to locate them either in the blind or seeing.

10. "There is no difference between the blind and seeing as regards the acuteness of the sense of smell.

11. "The blind to a greater degree get tired by manual labor than the seeing of the same age.

12. "The blind of the same age get tired quicker by manual labor than by mental work. This is not the case with the seeing of the same age.

13. "There is no essential difference in the degree of tiredness by mental work between the blind and seeing of the same age. Slight differences speak in favor of the seeing."

The general public are wont to attribute to those who are blind marvelous faculties of touch and hearing. In these respects the blind are regarded as abnormal and everything that is done by them is considered more or less wonderful.

On the contrary Mr. Kunz, a German educator of the blind, endeavors to establish in his booklet dealing with the Physiology of the Blind that persons deprived of sight have a less sensitive touch, and less acute hearing than persons with sight. The apparent contradictory fact that persons with sight are unable to read point print, which the blind read without difficulty, he tries to explain away by saying "that the finger of a person with sight having a very acute sense of touch will also feel the weaker impression of the letters of either side and will mix up these letters, i. e., not knowing which points belong to one and the same letter, whilst a finger not possessed of so acute a sense of touch will hardly

take notice of the weaker impressions produced by the letters of either side."

Believing that Mr. Kunz' premises and conclusions were incorrect and admitting that the sense of touch of persons with sight might be quite as sensitive as that of those without sight, I made a number of experiments with point letters placed at such a distance apart that the finger could cover not more than one letter at a time. The result proved that persons with sight perfectly familiar with the characters of the Braille alphabet were unable by touch, unaided by sight, to readily recognize the letters, whereas the blind person recognizes them without the slightest difficulty. In view of these experiments I came to the conclusion that while from a physiological standpoint the impressions made by touch upon the blind and seeing might be equal in strength and accuracy, the touch of the blind was psychologically reinforced by the mental faculties of attention and discrimination, hence they read with ease that which could not be deciphered through touch by the person with sight.

We frequently speak of the trained and educated eye, meaning thereby, not that the power of sight has been increased but that the power of observation has been acquired. In like manner the sense of touch is not keen until it has been taught to habitually observe. Our senses are not quickened by blindness, but they may be developed to a remarkable degree by careful and systematic training. Dr. Alexander Fraser says: "The senses, and indeed the whole nervous system are nothing more than a system of communication—in the psychological realm, communication between the world without and intelligence within. The mind is dependent for its development on the senses only in this sense and not absolutely. It is dependent on them much in the same way as we are dependent on our habits. We are helpless without our habits and find it most difficult to do things in new ways—but not impossible. The association between the mind and the senses as existing today is an *historical* one and a *good* one, but not the only one that might have been, and are we quite sure that it is the *best* one?

"It seems to me that in this wonderful plasticity of the human mind and its merely relative, historical dependence on the material senses, lies the hope of the blind, and it seems, too, that the most promising field for the realization of that hope is in the psychological study of not merely the sense phenomena but of the higher functions of the mind."

The intellectual superiority of the sense of touch as com-

pared with the senses of taste and smell is apparent, the impressions of the former being in all respects more clearly defined.

The power to retain in the memory sensations of touch is shown by the blind recognizing individuals after many years by shaking hands with them.

Prescott, the historian, was wont to say " that the world of the blind is circumscribed by the little circle which they can span with their own arms; all beyond has for them no real existence." I cannot concur with this view of the limitations of the world of the blind. From an intellectual standpoint Prescott disproved his statement, having extended the confines of his world so as to include Spain, Mexico, and Peru. Even from a physical standpoint the world of the blind is circumscribed by hearing rather than by touch.

Through the sense of touch blind persons acquire an unlimited amount of knowledge. They also enjoy many sensations of pleasure, and experience other sensations less agreeable. Smoothness and softness are sensations which excite emotions of pleasure, while hardness and roughness arouse emotions akin to pain.

Blind persons frequently avoid contact with trees and posts on the sidewalks, and with other objects in their homes through being made aware of such obstacles by the impression made upon their faces from the sudden condensation of the air. In walking we drive before us a slight wave of air. This is pushed on until it strikes a solid body, then the wave of air is thrown back upon the face and produces a slight sensation which is at once taken as a warning by the blind pedestrian. This facial sensation is not necessarily confined to persons who are blind but is occasionally experienced in the dark by persons with sight.

Sight has been well designated as the Queen of the Senses. Psychologically it is a characteristic of vision that it presents objects in synthesis while all the other senses represent them in parts.

Those who are blessed with sight receive much valuable knowledge subconsciously and in this respect are at a great advantage as compared with those deprived of sight.

To man in general light is regarded as one of the purest of organic gratifications. This is admirably voiced by the old Hebrew writer who says: " The light is sweet, and a pleasant thing it is for the eyes to behold the sun."

With the congenital blind it is impossible to imagine light. It is equally impossible for them to imagine color. Many

erroneous statements as to the ability of blind persons to distinguish colors by touch have been given wide publicity, but upon investigation it has been proved that the discrimination in selecting fabrics of different colors was dependent entirely upon the texture of the materials. The blind have learned by experience that the dyes used in various cloths make the surface hard, rough, smooth or silky. A red dye used upon certain material will always make the surface rough, whereas a green dye used upon a similar material will make its surface appear very smooth. It is therefore not surprising that blind persons have been credited with the faculty of telling color by touch. Color is a constant creation dependent entirely upon light, hence if the blind cannot see light they cannot see color and as they cannot recognize light by touch it is obvious that they cannot recognize color.

The sense of hearing is to the blind of the very highest value. Through it social intercourse with their friends is made possible. Through this sense they enjoy many of their greatest pleasures, such as the singing of birds, the hearing of music in all its forms and the sounds of nature with which they are surrounded. The sense of hearing coupled with the faculty of attention enables blind persons to move through the streets with comparative ease and safety. They note the vehicles that pass along the highway and can readily recognize the various sounds made by the tramcar, automobile, carriage or heavily laden dray. They likewise recognize those who may pass them on the sidewalk. To judge by the step on the pavement this one is carrying a heavy burden. The light step of a child is noticed, as is also the faltering step of an aged person. The trained ear learns to estimate distance, to judge direction and to be warned of a possible danger. Appreciation of the intensity and pitch of sounds has also proved most helpful. There is no need of further accentuating the value of hearing to the blind. It is the channel through which they may receive a very great part of their education. The possession of this sense enables the blind to follow many occupations and to achieve success in many walks in life. Every blind person thanks God for being able to hear.

I do not think that the sense of smell is developed in the blind to any remarkable degree, but with the deaf-blind its acuteness has enabled them to recognize odors which are absolutely insensible to other persons. Laura Bridgeman, the famous deaf-mute of the Perkins School for the Blind, would instantly recognize anyone that she had met before by the smell of his hand or glove. If she met a stranger

she invariably smelled his hand, and Dr. S. G. Howe asserts that the impression made upon her was so strong that she could recognize him long after by smelling his hand, or even his glove if just taken off. The blind deaf-mute, James Mitchell of Scotland, possessed a remarkable keenness of scent which enabled him to discover by smell the presence of a stranger in a room. It is said that he could point out the part of the room in which the stranger stood. Julia Brace, according to Dr. Howe's record, possessed the most acute sense of smell of anyone with whom he ever came in contact. "She smelled at everything which came within range of the sense and never forgot anyone with whom she had shaken hands."

It has been well said by Rousseau in contrasting taste and smell, that taste is the only sense which has nothing to say to the imagination. Odors excite the imagination and affect us not so much by what they furnish as by what they lead us to expect.

It is a somewhat remarkable fact that blind persons retaining the power to visualize always see in their dreams. These persons may be entirely blind and have been blind for many years, but in dreams they have perfect sight. The congenital blind when dreaming recognize no change between their waking and sleeping hours.

I trust as educators of the blind we will more and more realize that the congenital blind are dependent for first hand knowledge upon models of all kinds, simple and complex, and that their education along the lines of touch impressions should be pursued systematically. I also trust that we may fully realize the pleasures and advantages derived by a majority of the youthful blind from the power to visualize and that efforts will be made to discover means whereby this power of visualization may be strengthened and made perfect.

The psychology of the blind opens a wide field for experiment and investigation and I feel confident that in the future this special branch of psychology will receive more attention than it has in the past, and that as educators of the blind we will hail with pleasure any knowledge gained that may make the training of the youthful blind more practical and more successful.

In closing this paper I desire to acknowledge my indebtedness to many eminent psychologists from whose works I have drawn inspiration.

AN EXPERIMENTAL STUDY OF LITERARY VS. SCIENTIFIC TYPES[1]

By Gardner Murphy

The experiment here described had as its main objective the discovery of differences between subjects having a predominant interest in and aptitude for literature, and those having a predominant interest in and aptitude for science. In particular, it sought to discover diagnostic characteristics,—traits which invariably belong to one or the other group, and to its members only, so that the application of the test to a given subject would make possible, to some degree, a determination of his aptitudes. It was also hoped that the experiment would show certain well-defined differences between persons chiefly interested in aesthetic and emotional activities and persons chiefly interested in factual and practical activities. The study of this problem was in the main sacrificed to that of the first. Probably the data given by the experiment would help to answer both questions and several more, but only a small part of those data has been adequately studied. The results which are given by such experimentation seem to justify an intensive study rather than a superficial survey of the whole field.

The entire experimental work was done with the word-association test. The list used was the first hundred words of the Wells-Woodworth standard series,[2] printed with the usual spaces for entering data. The only other materials needed were stop-watch and pen and ink.

The procedure was as follows. When the subject had taken his seat on the opposite side of the table from the experimenter, he was given the following instructions: "I am going to pronounce a series of one hundred words. After each word please give the word which first comes to you, as quickly as possible. For example, if I should say *tennis,* you might immediately say *golf* or *racquet.* Only one word is desired, and it should be given as soon as possible after my word, without deliberation. If there is difficulty in hearing or un-

[1] An investigation carried out in the Yale Psychological Laboratory as part of the work in the advanced laboratory course, for seniors.

[2] Psychological Review, Monograph Supplement, Vol. 13, 1910-11.

derstanding any word, please let me know, and I will come back to it later. The eyes should be kept closed throughout the experiment. There will be a series of ten practice words before the actual experiment is begun. Is that all perfectly clear?" After any necessary explanations, the practice series was given, and the subject was asked for introspections. The experiment-series was then begun. The experimenter's eyes were on the stop-watch except while he was recording answers and association-times, for it was found more accurate to watch the continuously moving hand than to start and stop the watch for each word. The time recorded was the interval between the end of the stimulus-word and the beginning of the associate, measured in fifths of a second. If the subject gave no associate in five seconds, the word was passed; if he failed on several repetitions of the same stimulus (with other words intervening) it was called a failure. In computing averages of times, failures are classed as 25 (5 seconds). After completion of the series, introspections were called for, both for the experience as a whole, and for particular words whose introspective explanation seemed of peculiar value. The second trial was held at the end of an interval averaging forty days. The effect of variation in the length of the interval will be discussed later. The second trial differed from the first in that the instructions and practice series were omitted, the subject being asked simply whether he recalled the instructions, and helped out if he had partly forgotten them. It seemed sufficient if the subject remembered that he was to give the first word that came to him, without deliberation, and that he was to keep his eyes closed. Introspections were again asked for.

The subjects were forty in number,—twenty-seven seniors in Yale College, and thirteen members of the faculty. Of the students, eight were specializing in literature, nine in science, and ten in other subjects. They were, moreover, classified in five groups according to my estimate of their aptitudes and interests,—Group 1 those of the decidedly aesthetic and emotional type, Group 5 those of the decidedly practical type, with the other groups intermediate. The estimates were of course made before experimentation. They are far from infallible; but I have known these men intimately, and a friend's estimate is probably worth more than the fact that they were specializing in given branches of the curriculum. In some cases I asked the men how they would classify themselves, and took their answers into account in my decision. As a matter of fact only one subject "majoring" in literature was

put into the scientific end of the spectrum, and none "majoring" in science were transferred from that end. Of the members of the faculty, five were professors of English literature; eight were professors of science, distributed as follows: Anthropology, 1; Biology, 2; Chemistry, 1; Geology, 1; Mathematics, 1; Physics, 1; Psychology, 1. The professors of English I called Group L, the professors of science, Group S. I decided that there were so many ways in which the types might be studied that it would be well to pick out a small group of decided representatives of the two types, so that I could tell whether it would be worth while to apply a given method to the results of the whole forty;[3] for if positive types gave no valuable results, it seemed unlikely that less pronounced types would do so. These two special groups were composed of five members each, four members of the faculty and a student of unquestionably pronounced tendencies, —in the one case a man of very unusual literary capacity and feeling, in the other a very brilliant student of science. The literary group was called Group A; the scientific, Group B. The subjects are referred to throughout by initials.

Though the method of association-times did not seem promising, I thought it worth while to compare Groups A and B by this method. The results were as follows:

Gr. A	Av. time in 5ths Trial 1	Av. time in 5ths Trial 2	Improvement	Gr. B	Av. time in 5ths Trial 1	Av. time in 5ths Trial 2	Improvement
J.C.A..	4.74	4.29	9.5%	G.A.B..	8.64	7.27	15.9%
T.B....	9.02	5.76	36.1%	H.A.B..	5.05	3.58	29.1%
L.S.M..	6.31	6.02	4.6%	R.C.T..	5.99	4.52	24.5%
W.L.P..	5.17	4.84	6.4%	W.A.W.	6.61	4.66	29.5%
C.B.T..	5.67	5.08	14.1%	L.L.W..	6.60	5.63	14.7%
Av...	6.18	5.20	15.9%	Av...	6.58	5.13	22.0%

These figures justify no conclusions as to differences between the types, since the group-averages are so nearly equal. The scientific group shows greater improvement, but with T.B. showing more than any subject in group B, and W.L.P. and C.B.T. so nearly equal to L.L.W. and G.A.B., respectively,

[3] Several methods were so applied. Data from thirty-seven subjects are used; but the results from the three members of Group 3, the men whose tendencies in the two directions seemed about equal, were not significant enough to warrant inclusion here.

we obviously have no diagnostic characteristic. The results did not seem to justify the application of this method to the other subjects.

An investigation to discover whether there were marked differences between the groups in the number of *repetitions* (of the *same associate* to a *given stimulus* as it appeared in the two trials) gave the following results. (Changes from singular to plural in nouns, and vice versa, and changes from present to past tense in verbs, and vice versa, were counted as repetitions; otherwise only exact reproductions were allowed.)

Group A		Group B	
J.C.A.	32	G.A.B.	41
T.B.	38	H.A.B.	56
L.S.M.	21	R.C.T.	31
W.L.P.	39	W.A.W.	47
C.B.T.	50	L.L.W.	50
Av.	36	Av.	45

Here we have a distinct difference in the group-averages; and also, if we take our total average, 40.5, we find that four of Group B fall above it, and four of Group A below it. With only two atypical subjects out of ten, the method seemed to deserve application to the remaining subjects,—that is, to Groups L, 1, and 2, on the literary side, and Groups S, 4 and 5 on the scientific side, omitting those subjects who are also members of Groups A and B, and have been studied as such. The results:

Literary				Scientific			
M.J.B.	28	R.W.W.	31	R.P.A.	43	E.N.L.	45
D.N.B.	38	S.K.V.	30	J.B.	39	M.M.M.	38
J.M.B.	41			H.H.B.	48	G.A.M.	42
J.S.B.	28			H.C.B.	24	J.L.M.	31
E.R.B.	48			A.R.F.	43	D.A.Q.	41
F.W.L.	30			M.H.	39	E.S.R.	42
B.S.	38			D.U.H.	43	S.A.T.	33
C.R.W.	27			A.G.K.	45	W.W.	29
				F.K.	36		
		Av.	33.9			Av.	38.9

Here we have results of the same character, though not quite so pronounced. Before drawing conclusions, however,

I wanted to make sure that the varying interval between trials was not vitiating the significance of my results. A comparison of the five subjects having the longest interval with the five having the shortest interval gave these data:

Five longest	Period	Number repeated	Five shortest	Period	Number repeated
E.S.R......	53 days	42	H.C.B.....	31 days	24
B.S........	53 days	38	R.W.W....	31 days	31
S.A.T......	52 days	33	J.W.F.....	29 days	32
A.R.F......	50 days	43	D.U.H....	28 days	43
M.J.B......	49 days	28	E.N.L.....	26 days	45
Av.......	51.4 days	36.8	Av......	29 days	35

The average repetition is slightly *greater* when the period averages nearly *twice as long.* Since the individual variation is more than great enough to balance what losses take place in the interval from 29 to 51.4 days, such losses cannot be considerable. Returning to the results of the repetition-study, I think we are justified in concluding that the scientific subjects tend as a group to repeat a larger number of associates, but the intra-group variation is so large, and so many subjects are atypical, that we have no reliable diagnostic test herein. Of the 27 subjects studied in the latter comparison 18 were typical, 9 atypical. It will be remembered that the former comparison (Groups A and B) gave 8 typical to 2 atypical.

In the Kent-Rosanoff experiment,[4] tables were compiled showing the number of subjects giving each reaction-word which was associated with each stimulus. It seemed possible that the groups might vary in "community,"—that one group might give more commonplace words than the other, one group choosing obvious associates, the other abstruse associates. For this comparison I used the ten Kent-Rosanoff stimulus-words which appear in my hundred words from the Wells-Woodworth series. Using the results of both trials, I had thus 20 associates for each subject. The figures used in the Kent-Rosanoff experiment to measure community were the number of subjects out of a thousand who gave a particular associate to a given stimulus. The averages and medians[5] given below are derived simply from these figures for the twenty associates given.

[4] American Journal of Insanity, Vol. 67, 1910.
[5] The median is half the sum of the tenth and eleventh numbers.

Group A			Group B		
Subject	Average	Median	Subject	Average	Median
J.C.A........	28.98	154	G.A.B.......	31.24	90
T.B..........	24.94	22.5	H.A.B.......	40.97	173
L.S.M.......	16.93	26.5	R.C.T.......	29.44	59.5
W.L.P.......	24.78	55.5	W.A.W......	37.97	166
C.B.T.......	30.17	42.5	L.L.W.......	39.13	166
Av........	25.16	60.2	Av........	35.75	131

Here we have a decided difference between groups, the difference being more apparent by using medians. Taking the median[6] of the ten subjects' medians, or 74.8, and comparing each subject with this, we find eight typical, two atypical. This method was applied to the other subjects, with these results:

Average of "medians of community" for
10 literary subjects.......................................72.65
17 scientific subjects 90.03

The same difference is apparent, but is not large enough to be conclusive. A good deal of study of this problem by different combinations of averages and medians showed throughout an unsatisfactorily small margin. For example:

Average of "average of community" for
10 literary subjects...22.9
17 scientific subjects.......................................26.3

On the whole, I think it probable that we have here an indication of the same tendency that was so pronounced in the case of Groups A and B, but we cannot be positive, and we certainly have not a good diagnostic test. Another way of studying community was to count the number of individual words which were used by the groups in answer to each stimulus;—if all five members of Groups A and B give the same associate, we count one; if all gave different associates, we count five. So the group having the smaller total for the hundred words will have the larger community within itself,—i. e., less variation. Using the data of trial 1, this comparison gave the literary group 405, the scientific group 378. Again a larger scientific "community." All the evidence seems to be of the same kind, yet it is inconclusive. Taking this problem as a whole, I think we may say that a tendency to greater commonplaceness of associates on the part of the scientists is "highly probable."

[6] Half the sum of the fifth and sixth numbers.

Closely connected with the preceding comparison is the number of "individual reactions,"—associates not given by any other subject. This would mean of course, in my experiment, associates which do not appear at all in the Kent-Rosanoff tables. Group A gave 21 such associates, Group B gave 9. The 10 remaining literary subjects gave 29, or an average of 2.9; the 17 remaining scientific subjects gave 46, an average of 2.7. Again the results from the larger groups are not as positive in their indication of differences as are those from Groups A and B,—though similar in kind. *Probably* the literary subjects tend to more individual reactions,—a result in harmony with the conclusions as to "community."

We come now to classifications of the associations, and problems connected therewith, a much more hopeful, as also a much more difficult task. First, as to grammatical considerations pure and simple. Wreschner,[7] and more recently Sutherland,[8] have used the method of classifying associations on the basis of the parts of speech to which stimulus and associate belong. Since all the stimuli in my list fall under the heads *noun, adjective, verb,* and *adverb,* and answers may be in any of the parts of speech, I have thirty-two classes, as shown below. Three notes are necessary to an understanding of the classification. (1) There are in my list 58 nouns, 20 adjectives, 20 verbs, and 2 adverbs. (2) Some of the stimuli which are commonly used as nouns were apperceived by the subjects as verbs. My classification is on the basis of the way the subject apperceived them. A few cases leave this an open question, but they are so few as to allow no chance for serious error. (3) Where the totals do not equal 100, the difference is the number of the subject's failures. The following is a typical classification.

W.L.P. TRIAL 1

	Associates							
Stimulus	Noun	Adj.	Verb	Adv.	Pron.	Prep.	Conj.	Intj.
Noun	49	4	2	..	..	..	..	..
Adjective	6	16	..	..	..	..	..	..
Verb	8	1	11	1	..	..	..	..
Adverb	1	..	..	..	..	..	1	..

If we start at the upper left hand corner and read down and to the right along a diagonal (49-16-11), we shall have the

[7] Zeitschrift für Psychologie und Physiologie der Sinnesorgane; Ergänzungsband III, Theil 1 u. 2.

[8] Sutherland, A. H., A Critique of Word Association Reactions. 1913, Menasha, Wis., Geo. Banta Pub. Co.

symmetrical associations, where stimulus and associate are members of the same part of speech. Now compiling such tables for Groups A and B for the first trial, we have:

Group A. Averages

Stimulus	Associates								Totals
	Noun	Adj.	Verb	Adv.	Pron.	Prep.	Conj.	Intj.	
Noun	51.2	2.4	3.0	....	....	.2	....	....	56.8
Adjective	6.0	14.2	.8	....	....	....	....	....	21.0
Verb	6.0	1.4	10.0	.8	....	....	....	....	18.2
Adverb	.2	....	.4	1.8	....	....	.2	....	2.6
Totals	63.4	18.0	14.2	2.6	....	.2	.2	....	98.6

Total symmetry, 77.2; symmetry varies from 69 to 82

Group B. Averages

Stimulus	Associates								Totals
	Noun	Adj.	Verb	Adv.	Pron.	Prep.	Conj.	Intj.	
Noun	46.2	5.8	1.8	.2	....	....	....	.2	54.2
Adjective	4.4	15.4	.2	.4	....	....	....	....	20.4
Verb	8.2	2.6	11.0	.6	....	....	....	....	22.4
Adverb	....	....	....	1.6	....	....	.2	....	1.8
Totals	58.8	23.8	13.0	2.8	....	....	.2	.2	98.8

Total symmetry, 74.2; symmetry varies from 60 to 87

This is of course enough to show that symmetry is not a diagnostic character. But some of the grammatical relations show enough difference between the groups to justify further work with this method. I therefore added the results of trial 2 for Groups A and B, taking the *totals* for *each class* of association.

Group A

Stimulus	Associates							
	Noun	Adj.	Verb	Adv.	Pron.	Prep.	Conj.	Intj.
Noun	**521**	**18**	23	..	..	1	..	..
Adjective	**73**	**123**	4	..	..	..	..	..
Verb	**98**	13	**87**	8	..	..	..	..
Adverb	2	1	3	11	..	..	2	..

Group B

Stimulus	Associates							
	Noun	Adj.	Verb	Adv.	Pron.	Prep.	Conj.	Intj.
Noun	**455**	**61**	23	2	..	..	..	2
Adjective	**38**	**164**	2	3	..	..	..	..
Verb	**68**	19	**126**	7	..	..	..	..
Adverb	..	..	2	15	..	1	2	..

We have here three well-defined differences. (1) When nouns are stimuli, the scientific group gives a very much larger number of adjective-answers than does the literary group; the proportion of noun-adjective to noun-noun associations is for the scientists 61/455, or 13.4%; for the literary people, 18/521, or only 3.5%. (2) When adjectives are given as stimuli, both groups give more adjective-answers than noun-answers, but the proportions are strikingly different; for the literary subjects there are 59.3% as many adjective-noun as adjective-adjective associations, for the scientists, 23.2%. (3) When verbs are stimuli, the literary subjects give slightly more noun-answers than verb-answers, while the scientific subjects give nearly twice as many verbs as nouns, the proportions of verb-noun to verb-verb associations being 112.6% and 54%, respectively. These figures are much more promising for the purposes of a search for diagnostic characters. I decided to apply each of these three criteria to each member of Groups A and B, to find out whether the variation within the groups was so large as to vitiate the significance of the totals. I counted a subject typical in those characteristics in which his percentage approximated his group average, atypical in those in which he was nearer the average of the other group. In order to have exact standards to judge by, I called a subject undecided (marked with a dash—) if there was a difference of less than 5 between his percentage and the percentage marking the *geometrical mean* between the averages of the two groups, typical if his percentage varied by more than 5 from this mean in the direction of his group, atypical if he varied by more than 5 in the opposite direction. For example, if Group A had a percentage of 15 in a given proportion of two kinds of associations, and Group B had a percentage of 60, a member of Group A would be called typical if his percentage were between 0 and 25, undecided if between 25 and 35, atypical if above 35. This method gave the following results. (t=typical, a=atypical.)

	Group A				Group B		
	Noun-stim.	Adj.-stim.	Verb-stim.		Noun-stim.	Adj.-stim.	Verb-stim.
J.C.A.	–	a	a	G.A.B....	t	a	t
T.B......	t	t	–	H.A.B....	a	t	t
L.S.M...	t	t	t	R.C.T....	t	t	a
W.L.P....	–	t	t	W.A.W...	t	a	t
C.B.T.....	t	a	t	L.L.W. ..	a	t	t

This means that every subject except J.C.A. has a majority of the typical characteristics of his group. In other words,

if we used these three criteria on subjects unknown to us, we could at this rate diagnose correctly in nine cases out of ten. Of course a failure in one case means that we have not a perfect test. J.C.A. is not a decided type, as he has a great interest in physics, and in scientific and factual things in general, but this is of course rather a lame excuse. The fact that the test works in nine cases means, however, that we have a good working-basis for a thorough-going diagnostic test. And there is no question that we have general tendencies here which are very pronounced, as the totals showed. If time had allowed, I think this method would have merited further study.

The customary classification of word-associations has been confined to from five to fifteen *classes,*—all the rest being called "miscellaneous." One classification gave 44% of the associates as "miscellaneous." Even Miss Fürst's classification,[9] which is as far as I know the most thorough-going yet made, gives only 15 classes, and the Wells-Woodworth classification[10] gives only five, with one of these subdivided into five. The usual classification is something like this:

1. Definition.
2. Predicate adjective.
3. Subordination.
4. Supraordination.
5. Speech-habit.
6. Defective reactions.
7. Miscellaneous.

In view of the vast number of possible ways of associating words, such a classification must throw into its last category not only many associations, but many *groups* of associations, coördinate with those given, and often equally, or nearly equally, important. It seems to me that the *large number of miscellaneous associations* is not due entirely to the *difficulty of deciding* where to put the associations, but in part to *the failure to separate* perfectly clear-cut classes as such, and to treat them as a part of the material used in computing results. For example, the association *lion-tiger* is common and normal; why should we call it "miscellaneous" any more than *lion-animal,* which we call "supraordination?" Accordingly, some classifiers have added the class "coördination" to include such associations. It seems to me that an extension

[9] In article by Jung, American Journal of Psychology, Vol. 21, April, 1910.
[10] Psychological Review, Monograph Supplement, Vol. 13, 1910-11.

of this principle will make the association-test a good deal more useful. The task is to make the new classes and add them on, cutting down the miscellaneous group. This does not mean necessarily the grouping of any associations whose true category is *questionable;* it means only that the groups which *are* definite be labeled and defined as separate groups. There will of course always be a miscellaneous group, because there are always associations which may have come about by any one of two or more associative processes. The classifier must be ready to put associations into this class if their nature is open to serious question. It is well to have two or more persons classify the results, and throw out those associations about which there is disagreement. But where the true category is not open to serious question, I would make the classes as many in number as need be to classify all answers which *can* be accurately classified.

Accordingly I set out to make a new classification. I shall first give it as it stands[11] entire, and then explain it, in so far as a thing so fearfully and wonderfully made can be explained.

I. Meaning-associations.
 1. Contiguity.[12]
  A. Spatial.
   a. Contiguity of separate things: bottle-cork, burglar-window.
   b. Selection of part of situation presented by stimulus: bottle-neck, cap-lining.
   c. Enlargement of situation presented by stimulus: table-room, blood-wound.
   d. Locating the situation presented by stimulus: omelet-Commons, music-Woolsey.[13]
  B. Temporal.
   a. Contiguity of two concepts, neither containing an idea of time, but associated by time-contiguity: death-doctor, dismay-disaster.
   b. Association of a concept with a time-idea.
    a. Locating in time the situation presented by stimulus: gift-Christmas, fish-Friday.
    b. Naming of a situation appropriate to a time-idea in the stimulus: occasion-speech, night-steal.

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[11] Following the name of each class. I give two illustrations, with a brief explanation in a foot-note, where this seems necessary. The illustrations are from the actual data of the experiment, where these could readily be found.

[12] Of course this refers to contiguity *within the situation* indicated by the pair, not to the psychological origin of the association. In the same way, Class 3, "opposites" is not meant to imply that this form of association is not reducible to a simpler form; and so on with the rest of the classes.

[13] Refers to Woolsey Hall.

2. Similarity.
   A. Synonyms.
      a. Exact: snake-serpent, purpose-aim.
      b. Approximate.
         a. Same part of speech and coordinate in extension:[14] delicate-fine, prefer-choose.
         b. Same part of speech, but not coordinate in extension.
            —. Increased extension:[15] indecent-bad, crime-harm.
            =. Decreased extension:[16] prospect-landscape, true-straight.
         c. Different part of speech; expensive-cost, end-last.
   B. Members of a class having a quality in common: produce-concoct, tiger-lion.
   C. Meanings more distantly related but distinctly similar: thick-heavy, dark-black.
3. Opposites.
   A. Exact: expensive-cheap, rich-poor.
   B. Approximate:[17] unfair-justice, unseen-visible.
4. Members of a common pair, not opposites.
   A. Associated by contiguity:[18] lightning-thunder, tar-feathers.
   B. Associated by similarity: man-woman, eating-drinking.
5. Subordination: instrument-flute, satisfy-pay.
6. Supraordination: cedar-tree, nourish-provide.
7. Cause and effect.
   A. Reaction-word denoting cause: stretch-tired, indecent-exposed.
   B. Reaction-word denoting effect: pinch-scream, bashful-blushing.
8. Particular person or thing as representative of a general idea.[19]
   A. When stimulus is a noun:[20] music-piano, ham-Armour.
   B. When stimulus is an adjective:[20] tough-beef, perverse-devil.
   C. When stimulus is commonly used as a noun, but used by the subject as though it were an adjective: ham-omelet, hip-joint.
9. General idea of which a particularized stimulus is representative (reverse of 8): acid-chemistry, fish-Christianity.
10. Substance of which stimulus is composed:[21] omelet-egg, rope-strands.

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[14] "Coordinate in extension" signifies that neither term includes the other.

[15] "Indecent" is a kind of "bad"; "bad" has greater extension.

[16] "Straight" is a kind of "true"; "true" has greater extension.

[17] Where association is by opposites, but the meanings are not exact opposites.

[18] Lightning and thunder are so intimately associated as to be a "familiar pair associated by contiguity."

[19] General idea "music" makes the subject think of a particular thing representative of it.

[20] There seems to be no great difference between 8A and 8B psychologically.

[21] Construed rather broadly.

11. Noun-stimulus delimited by
    A. Adjective:[22] mountain-bald, house-white.
    B. Participle: ham-hanging up, burglar-caught.
    C. Noun used as adjective: instrument-drawing, wagon-delivery.
12. Adjective-stimulus delimited by adverb: good-very, perfect-absolutely.
13. Verb-stimulus delimited by adverb: discourse-fluently, follow-near.
14. Selection of essential attribute of stimulus.[22]
    A. In adjective-reaction: tar-thick, mischief-bad.
    B. In noun-reaction: table-flatness, night-darkness.
    C. In participle-reaction: rope-twisted, tank-drunk.
15. Stimulus as subject of reaction-verb: lightning-strike, fish-swim.
16. Stimulus as object of reaction-verb.
    A. Direct object: fish-catch, umbrella-raise.
    B. Cognate accusative: gift-give, ditch-dig.
17. Stimulus as object of action shown by naming agent: house-breaker, school-visitor.
18. Stimulus verb with reaction as object.
    A. Direct object: smoke-cigarette, drink-water.
    B. Cognate accusative: dig-trench, run-race.
19. Stimulus-verb with reaction as subject: nourish-food, drift-snow.
20. Action appropriate to situation denoted by stimulus.
    A. Reaction in the form of a verb: prospect-look, lecture-listen.[23]
    B. Reaction in the form of a noun: death-despair, grocery-orders.
21. Thing appropriate to situation denoted by stimulus; salute-cap, drive-whip.
22. Reversal of point of view in verb-action:[24] satisfy-enjoy, enjoy-please.
23. Translation into other languages: sailor-matelot, overcoat-Überrock.
24. Reaction as the subject of action *implied* in noun-stimulus: instrument-engineer, dismay-man.
25. Situation appropriate to action denoted by stimulus:[25] ride-trolley, salute-battery.
26. Adverb stimulus modifying verb-reaction: again-ran, also-followed.
27. Adjective-reaction referring to same situation as adjective-stimulus: dark-cold, deep-salt.

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[22] Since *some* houses are *not* white, the subject is delimiting his stimulus; but since *all* tar is thick, he is in this case not delimiting it, but selecting an essential attribute.

[23] When there is a lecture one (usually) listens; the two are almost a plain case of contiguity, but I preferred a separate class for such characteristic reactions.

[24] If A satisfies or pleases B, B enjoys it. We simply change our point of view.

[25] *Battery* is not the direct object of *salute,* but it is the situation in which the subject was used to saluting.

28. Stimulus as source of reaction: tree-sap, grocery-provisions.
29. Reaction as source of stimulus: ham-pig, rain-clouds.

II. Pure verbal associations.
  1. Word-contiguity.
    A. In associated order.
      a. Completion of phrase: snake-charmer, glory-hallelujah.
      b. With intermediate words omitted: exchange-robbery,[26] captain-soul.
    B. In reversed order.
      a. Completion of phrase: cover-under, path-tow.
      b. With intermediate words omitted: lamb-Mary, burning-boy.
  2. Words of a common class:[27] also-not, again-since.
  3. Sound-similarity.
    A. Reaction beginning with same sounds: produce-procure, also-allow.
    B. Reaction ending with same sounds.
      a. Rhymes: hash-dash, rich-ditch.
      b. Others: occasion-vocation, scramble-humble.
    C. Other sound-similarities: barrel-arrow, elephant-aliment.

III. Unclassified: barrel-song, hip-gun.

Note 1. I have had to give much thought to the question of "logical" vs. "psychological" classification. I think the dilemma is often represented as worse than it really is, but there is nevertheless a real dilemma;—if we classify "logically," we may miss a great part of the psychological significance of the results; if we classify "psychologically" the large personal equation of the classifier comes in, and he may grossly misconstrue his material, so as to get meanings which are the fabric of his own rather than of the subject's mind. I think part of the trouble is due, however, to a confusion of a *classification of logical relations* and a *logical classification of psychological relations*. I have tried to work out the *latter* in detail (hence the sub-heads, etc.); and I have attempted to remove the danger of the personal equation (and of false classification) by allowing a large unclassified group,—cases, in which, to use my previous phraseology, the true classification was open to serious question.[28] This group also includes the reactions which are obviously reverberation-effects or answers to previous stimuli, as I classified in each case on the basis of the relation of the reaction-word to its stimulus.

[26] "Fair exchange is no robbery."

[27] The subject seems to think of the stimulus as a short common particle, not as a meaning.

[28] Whether I have made this group large enough is nevertheless a real question. As I look over the illustrations given after each class, as compiled six months ago, I find some which seem to me quite capable of different interpretations. Some of those, however, which appear doubtful, were rendered fairly certain by the introspections.

Note 2. Where it is not specified whether stimulus or associate is referred to, the latter is to be understood; e.g., "8, particular thing as representative," etc., "*reaction-word* denotes a particular thing," etc.

Note 3. I tried to follow some kind of system in the order of classes in "I. Meaning-associations," and did so up to No. 23; the other classes were added one by one, as members appeared during the work.

Note 4. Of course the classification does not claim to be complete. It aims only to cover all the classifiable associations in *this experiment* with *these subjects.* In fact, I often saw where there were possible associates falling under classes or sub-classes in which no actual associates were given. It is distinctly a tentative list, and is offered as a suggestion. I am certain that it contains many imperfections, and hope that it will be critically tested and amended.

Note 5. By "meaning-associations" I designate associations in which the meaning of the two words is the reason for their association, as distinct from their form as words. They are immensely in the majority. Where the *words as such,* not their meanings, are associated, I call the process pure verbal association. For example, to the stimulus "watchful" the answer "shepherd" is an illustration of the former; "waiting," of the latter. All of the "verbo-motor" category belong, of course, under my second main division, but I did not designate such a class, because it seemed impossible to tell whether the association of two words in this way is really motor. A good many subjects get visual images, and it is quite possible that these determine the associations. If the classifier means by "verbo-motor" simply that one word as such (not a meaning) causes the pronouncing of another, there is no difference between the old classification and the new except that the new one is subdivided into several classes. I think, however, that the "verbo-motor" category has usually meant simply "speech-habit." And I do not see how any one can be sure that such an association as *merry-widow,* for example, is not as much due to seeing the words together as to pronouncing them together. I think it is a mistake to allow any words in the "verbal" group unless it is certain that they are due to such purely verbal associations, for many an apparent verbal association may be improvised as a meaning-association. I decided, therefore, (1) to class words on their meaning-relation where that seemed of primary importance in the actual determination of the answer which the subject gave, (2) to class them as verbal associations when, and only

when, they showed distinct evidence of such character, (3) to allow a large unclassified group for words as to whose true status I was uncertain. E. g., *black-white* would be classed as a meaning-associate, in spite of some probable pure-verbal quality; *watchful-waiting* would count as a verbal association, while *crime-punishment* would be unclassified.

Note 6. It will be observed that the classification aims to consider always what is in the subject's mind, in so far as this can be ascertained. Introspections were of course of great help in some cases where the relation of the two words was obscure. Since the classification aims in this way to be "psychological," I have not hesitated to use the word "situation" in the sense of the whole mass of imagery which the stimulus brings to the subject's mind. I hope that the word has not been used too loosely, and that I have not supposed there were given elements in the "situation" unless I had evidence for it.

I first took the reactions which the subjects of Groups A and B gave on both their trials, i. e., the "perfect reproductions" (allowing, as before, changes in tense and number), about 10×40, or 400 associations.[29] I thought these would probably be more sure to be typical of the subjects. The results are given in the following chart.

The results show definite differences between the groups in the following respects:

(1) The number of associations by contiguity is much larger for the literary subjects, the totals being 30 to 17; though the "contiguity of separate things" shows a slight advantage for Group B, it is more than balanced by the other three classes.

(2) Group B shows many more associates which are "members of a class having a quality in common with the stimulus," the comparison giving them 24 to Group A's 10.

(3) Group B has over twice as many opposites, having 46 to Group A's 19 exact ones, and 5 to Group A's 1 approximate one, or a total of 51 to 20.

(4) "Members of a common pair associated by similarity" show 26 for Group B, 7 for Group A.

(5) Literary subjects show not a single associate delimiting a noun-stimulus, while scientific subjects show 6.

(6) Group A has 10 associates which name the object of a stimulus-verb; Group B has but 4.

(7) Group A has only 1 "word-contiguity" association; Group B has 10.

[29] Actually a few less, on account of failures.

	Group A						Group B					
	J.C.A.	T.B.	L.S.M.	W.L.P.	C.B.T.	Totals	G.A.B.	H.A.B.	R.C.T.	W.A.W.	L.L.W.	Totals
I 1 A a	1	3	..	1	2	7	3	1	..	5	..	9
b	2	5	2	3	3	15	4	1	1	..	2	8
c	..	1	1	1	2	5	..	..	..	..	..	0
d	..	3	..	..	..	3	..	..	..	..	..	0
I 1 B b a	..	..	..	..	..	0	..	..	..	1	..	1
a	..	1	..	..	..	1	..	..	1	..	..	1
b	..	..	..	..	..	0	..	..	..	..	..	0
I 2 A a	3	3	..	4	6	16	1	6	..	4	4	15
I 2 A b *a*	1	7	2	6	2	18	2	1	1	2	4	10
b —	..	..	..	1	3	4	1	2	1	3	1	8
b =	..	..	..	1	1	2	..	..	..	..	..	0
c	..	..	..	..	..	0	..	..	..	..	..	0
I 2 B	2	3	..	4	1	10	2	9	1	3	9	24
I 2 C	1	..	..	..	1	2	..	..	..	..	..	0
3 A	3	2	..	4	10	19	5	16	7	9	9	46
3 B	..	..	..	1	..	1	..	1	..	1	3	5
4 A	2	1	1	1	2	7	..	1	..	3	1	5
4 B	2	1	1	2	1	7	3	9	4	4	6	26
5	..	2	..	..	2	4	..	1	..	..	1	2
6	4	..	1	..	..	5	..	1	2	..	1	4
7 A	..	..	..	..	..	0	..	..	..	..	..	0
7 B	..	..	..	..	1	1	..	..	..	..	..	0
8 A	1	1	..	1	2	5	1	..	..	..	1	2
8 B	..	3	2	1	1	7	2	..	1	1	..	4
8 C	..	..	..	..	..	0	1	..	..	1	..	2
9	..	1	..	..	..	1	..	..	..	..	1	1
10	..	..	..	1	3	4	2	1	1	..	1	5
11 A	..	..	..	..	..	0	1	..	3	1	..	5
11 B	..	..	..	..	..	0	..	..	..	..	..	0
11 C	..	..	..	..	..	0	..	..	..	1	..	1
12	..	..	..	..	..	0	..	..	..	..	..	0
13	..	..	..	1	..	1	..	..	..	..	..	0
14 A	..	..	..	..	1	1	1	1	..	..	..	2
14 B	..	..	..	..	..	0	..	..	..	..	..	0
14 C	..	..	..	..	..	0	..	..	..	..	..	0
15	1	..	..	..	..	1	2	..	..	..	..	2
16 A	..	..	..	..	..	0	..	..	..	..	..	0
16 B	..	..	..	..	..	0	3	..	..	..	1	4
17	..	..	..	..	..	0	..	..	..	..	..	0
18 A	2	2	2	1	3	10	..	1	1	2	..	4
18 B	..	..	..	..	..	0	..	..	..	..	..	0
19	1	1	3	..	1	6	..	1	1	..	1	3
20 A	..	..	..	..	..	0	..	..	..	..	..	0
20 B	..	..	..	..	..	0	..	..	..	..	..	0
21	..	..	..	1	..	1	..	..	..	..	..	0
22	..	..	..	..	..	0	..	..	..	..	..	0
23	..	..	..	..	..	0	..	..	..	..	..	0
24	..	..	..	..	..	0	..	..	..	..	..	0
25	..	..	..	..	..	0	..	..	..	..	..	0
26	..	..	..	..	..	0	..	..	..	..	..	0
27	..	..	..	..	..	0	..	..	..	..	..	0
28	..	..	..	..	..	0	..	..	..	..	..	0
29	..	..	..	..	..	0	..	..	..	..	..	0
II 1 A A	1	..	..	..	..	1	3	1	2	3	1	10
b	..	..	..	..	..	0	..	..	..	..	..	0
II 1 B a	..	..	..	..	..	0	..	..	..	..	..	0
b	..	..	..	..	..	0	..	..	..	..	..	0
II 2	..	..	..	..	..	0	..	..	1	..	1	2
II 3 A	..	..	..	..	..	0	..	..	..	..	..	0
3 B a	..	..	..	..	..	0	..	..	..	..	..	0
b	..	..	..	..	..	0	..	..	..	..	..	0
3 C	..	..	..	..	..	0	..	..	..	..	..	0
III	5	2	6	4	2	19	4	3	3	3	2	15

These results seemed to me distinctly promising in the search for a diagnostic test. It will be noted that I have included no results in which there was not an absolute difference of at least 5, and a proportional difference of at least 50% (larger number half again as large as smaller number). In every case but the first one, the larger number is more than twice as great as the smaller. I now applied the same method of classification to the reactions which were not repeated,—the associates given in either trial 1 or trial 2, but not both. This meant the classification of about 1,200 more associations. (The average number of repetitions being 40, the average number of non-repeated associations from one subject is 200 —[40×2]=120). I give below the *sum* of the results for repeated and non-repeated associations. The results for the repeated associations were of course doubled before being added to the others.

The following results are apparent:

(a) Every one of the four classes in space-contiguity and of the three in time-contiguity shows a balance in favor of Group A. The total number of contiguity-associations is, for Group A, 175, for Group B, 91.

(b) The number of synonyms is about the same for the two groups, this applying to the separate classes as well as to the total number of synonyms.

(c) Group B has 91 reactions of "members of a class having something in common with the stimulus;" Group A has but 54.

(d) Group B gives 132 exact opposites to 66 for Group A, just twice as many, and 34 approximate opposites to Group A's 9,—a total of 166 to 75.

(e) Group B gives 62 "members of a common pair associated by similarity;" Group A, 22.

(f) Group A leads by a good margin in "subordination," having 44 to 25 for Group B, and in

(g) "Supraordination," having 23 to Group B's 13.

(h) When adjective-stimuli are given, literary subjects are much more likely to give an answer naming something appropriate to that adjective; the numbers are 49 to 23, respectively.

(i) On the other hand, when noun-stimuli are given, scientific subjects are much more likely to delimit it by an adjective (22 to 5),

(j) And they are more likely to select an essential attribute (19 to 11).

(k) Literary subjects give more nouns as the subjects of stimulus-verbs (27 to 19).

	Group A						Group B					
	J.C.A.	T.B.	L.S.M.	W.L.P.	C.B.T.	Totals	G.A.B.	H.A.B.	R.C.T.	W.A.W.	L.L.W.	Totals
I 1 A a	9	15	15	10	13	62	13	3	5	12	6	39
I 1 A b	12	14	6	9	19	60	11	7	10	5	4	37
I 1 A c	..	7	5	3	4	19	1	1	2	1	..	5
I 1 A d	..	10	9	1	..	20	3	..	..	..	1	4
I 1 B a	..	..	3	2	1	6	..	..	..	4	..	4
I 1 B b *a*	..	4	3	..	..	7	..	..	2	..	..	2
I 1 B b *B*	..	..	..	1	..	1	..	..	..	..	..	0
I 2 A a	11	7	5	13	16	52	4	16	5	11	14	50
I 2 A b *a*	15	25	7	19	12	78	12	13	9	13	24	71
I 2 A b *b* —	11	4	2	8	9	34	6	5	5	8	7	31
I 2 A b *b* =	4	5	..	2	2	13	3	1	1	3	3	11
I 2 A b *c*	2	3	..	1	4	10	1	1	2	3	..	7
I 2 B	11	13	7	17	6	54	9	25	4	16	37	91
I 2 C	6	3	4	..	3	16	6	..	..	1	3	10
3 A	14	10	2	15	25	66	20	41	20	27	24	132
3 B	2	2	1	4	..	9	..	10	2	6	16	34
4 A	4	3	4	3	4	18	..	4	..	6	5	15
4 B	5	4	4	6	3	22	9	21	10	9	13	62
5	1	10	13	11	9	44	5	6	5	4	5	25
6	11	6	2	3	1	23	..	4	4	2	3	13
7 A	..	1	1	..	..	2	..	..	2	1	..	3
7 B	2	1	3	1	5	12	..	3	..	..	1	4
8 A	3	7	6	10	7	33	7	3	6	4	7	27
8 B	1	14	18	9	7	49	10	..	5	7	1	23
8 C	..	..	..	..	1	1	2	1	3	3	..	9
9	..	2	1	..	1	4	..	1	..	..	2	3
10	1	1	1	3	9	15	5	2	5	1	2	15
11 A	2	..	1	2	..	5	2	..	16	4	..	22
11 B	1	..	2	..	..	3	..	..	..	..	..	0
11 C	..	1	..	..	..	1	..	..	..	3	..	3
12	..	..	..	..	..	0	..	..	..	..	..	0
13	2	..	..	2	..	4	1	..	3	..	..	4
14 A	2	..	1	4	2	9	5	3	7	1	..	16
14 B	..	..	..	..	1	1	..	..	..	..	..	0
14 C	1	..	..	..	..	1	1	..	1	1	..	3
15	2	2	..	..	..	4	5	..	..	..	..	5
16 A	1	..	..	1	1	3	..	..	..	1	1	2
16 B	2	..	..	..	..	2	7	..	..	1	2	10
17	..	..	..	..	..	0	..	..	..	..	..	0
18 A	5	11	13	9	10	48	10	5	14	7	4	40
18 B	1	..	..	..	1	2	..	..	..	..	..	0
19	2	4	11	5	5	27	2	3	6	6	2	19
20 A	2	2	1	..	..	5	..	..	..	..	..	0
20 B	..	..	..	..	..	0	2	..	..	..	..	2
21	1	1	..	2	..	4	..	..	..	..	..	0
22	1	1	..	..	1	3	..	..	1	..	..	1
23	..	..	..	..	..	0	..	..	..	..	..	0
24	..	..	3	..	..	3	..	..	..	..	..	0
25	..	1	1	..	..	2	2	..	1	1	..	4
26	..	..	1	..	..	1	..	..	..	..	..	0
27	..	2	..	..	..	2	..	1	..	..	..	1
28	..	..	..	..	..	0	..	1	..	..	..	1
29	..	1	..	2	1	4	..	1	..	..	..	1
II 1 A a	7	..	6	..	1	14	9	3	8	6	3	29
II 1 A b	1	..	..	..	..	1	..	2	2	..	..	4
II 1 B a	1	..	1	..	..	2	..	..	..	..	..	0
II 1 B b	..	..	..	..	..	0	..	..	..	..	..	0
II 2	..	..	..	..	..	0	..	..	2	..	2	4
II 3 A	2	1	3	..	..	6	1	..	..	..	..	1
II 3 B a	2	..	..	..	..	2	..	..	..	..	1	1
II 3 B b	..	..	..	..	..	0	..	..	..	..	..	0
II 3 C	..	..	..	..	..	0	..	..	..	..	..	0
III	34	11	33	19	14	111	16	15	28	19	15	93

(1) Scientific subjects give over twice as many "word-contiguity" associations completing a phrase (29 to 14),—the commonest class of the pure verbal combinations.

Although I have drawn no conclusions except where there were considerable margins, I think conclusions (g), (i), (j), and (k) above had better be thrown out, because the variation within the group is so large in comparison with the difference between the group-averages. The others, except of course (b), are, I believe, clear-cut differences between the types, large enough to be of value in establishing group-differences. The following is an application of these results to the ten subjects, to find how good a diagnostic test we have here. "a" and "t" are again used as markers, the criterion being as follows: a geometrical mean is taken between the two group-averages; if the difference between the subject's rating and this mean is less than 10% of the magnitude of the latter, he is called undecided; if he varies more than this he is marked on the basis of the direction of his variation, whether toward his group or away from it. If Group A averages 20, and Group B 80, a literary subject less than 36 is typical, from 36 to 44 undecided, above 44 atypical. The headings of the columns below refer to the conclusions as to group-differences which we have just drawn.

Group A	(a)	(c)	(d)	(e)	(f)	(h)	(l)	Net results
J.C.A.	a	t	t	t	a	a	a	atypical
T.B.	t	–	t	t	t	t	t	typical
L.S.M.	t	t	t	t	t	t	a	typical
W.L.P.	–	a	t	t	t	t	t	typical
C.B.T.	t	t	a	t	t	–	t	typical
Group B								
G.A.B.	a	a	a	t	t	a	t	atypical
H.A.B.	t	t	t	t	–	t	a	typical
R.C.T.	t	a	–	t	t	t	t	typical
W.A.W.	t	t	t	t	t	–	t	typical
L.L.W.	t	t	t	t	t	t	a	typical

Here we have eight subjects typical, two atypical, and these two only by a margin of four atypical to three typical characters. But of course the test is unsatisfactory as long as it makes two subjects out of ten atypical. I therefore decided to amend it by omitting any characteristics which showed more than three atypical subjects out of ten. This means throwing out the last one, column (1). By doing so we find J.C.A. of undecided status, 3 to 3, and G.A.B. still atypical. But it means that not a single other subject has more than one atypical characteristic out of six, and five

have no atypical characteristics. Class (e), "members of a common pair associated by similarity," gives us "typical" in all ten subjects; it is therefore the best diagnostic test we have yet encountered. Class (f), "subordination," gives eight typical, one atypical, and one undecided. The application of these two criteria alone would give us nine typical subjects, and one undecided,—J.C.A., whom we found to be the one atypical subject in our classification on the basis of the parts of speech.

Some experimenters have gone into the matter of reactions by proper names, and have attached considerable significance to such reactions. This method was applied to Groups A and B, with these results:

Signification of proper name

Group A	Persons	Nations	Cities	Places not cities	Times	Physical things	Misc.	Total
J.C.A. . .	0	0	0	0	0	0	0	0
T.B.	0	0	1	1	2	0	0	4
L.S.M. .	8	0	3	9	2	1	0	23
W.L.P. . .	1	0	0	0	0	1	0	2
C.B.T. . .	1	0	0	1	0	0	1	3
Totals.	10	0	4	11	4	2	1	32
Group B								
G.A.B. . .	3	0	2	0	0	1	0	6
H.A.B. . .	0	1	0	0	0	0	0	1
R.C.T. . .	1	0	0	0	0	0	1	2
W.A.W. .	0	0	0	0	0	0	0	0
L.L.W. . .	0	0	0	0	0	0	0	0
Totals.	4	1	2	0	0	1	1	9

Though we have of course no diagnostic character here, the great mass of proper names from L. S. M., who has been so distinctly typical heretofore, made me think it worth while to try this method with the other subjects. The totals for the groups were as follows:

Signification of proper name

	Persons	Nations	Cities	Places not cities	Times	Physical things	Misc.	Totals
Literary . . .	32	0	0	12	1	0	7	52
Scientific. .	11	2	1	11	3	0	5	33

With *ten* literary subjects giving 53 proper names to 33 for *seventeen* scientific subjects,—an average per person of

5.2 against 2.0, there seems no question that we have a real difference in group-tendencies. The personal variation is too large to make the test of great value in individual cases, however clear the results might be as to the groups. A glance at the results shows that the difference between the groups lies in the proper names which refer to *persons,* the others being about equally distributed between the two groups. I therefore set out to see whether the literary subjects gave more reactions-in-general referring to persons, aside from proper-name answers. Do they use many words like *soldier, baby, architect, mother,*—words signifying *people?* This study was made first by means of the associations from adjective-stimuli,—to see whether adjectives given were made by the subject to refer to persons or things. I did not get enough material here to justify any conclusions, so I undertook the matter on a larger scale, counting all the common-noun reactions from Groups A and B. The results:

	Signification of noun						
	Physical things	Non-physical things	Numerals	Abstract nouns	Human beings	Animals	Unclassified
Group A							
J.C.A	29	9	0	2	4	5	0
T.B	38	12	1	0	7	3	1
L.S.M	36	7	1	1	14	2	0
W.L.P	39	8	0	1	11	4	1
C.B.T	46	17	1	1	3	3	3
Totals	188	53	3	5	39	17	5
Group B							
G.A.B	31	9	0	1	10	3	2
H.A.B	33	11	0	2	7	1	0
R.C.T	39	5	0	1	9	0	2
W.A.W	38	14	0	1	7	4	1
L.L.W	31	12	0	2	5	3	0
Total	172	51	0	7	38	11	5

A fine case of absolutely negative results. Apparently the tendency to refer to *persons,* which characterizes Group A, is limited to *particular persons,* for the *common-noun* reactions which refer to persons are evenly distributed.

Jung speaks of predicate-types and definition-types;[30] the former react to stimuli emotionally, the latter intellectually. He considers the types well-defined and permanent,—that is, the members of a type stay in that type on all trials. The

[30] American Journal of Psychology, Vol. 21, April, 1910.

Kent-Rosanoff experiment yielded some very interesting results in this field, but the experimenters were not sure as to the permanence of the types,—whether individuals would show the same tendencies in later trials. It seemed to me that Jung was probably right, and that there might be something here of value for my purposes; it might be that the literary people, interested in aesthetic and emotional things, would tend toward predicates, and the scientists, interested in factual things, toward definitions. I found to my surprise that on this basis all my subjects were scientists,—all had more definitions than predicates, and actually the *scientists* had more *predicates* than the literary people. Though the results throw no light on my problem, they are interesting as showing extreme personal variation in the number of predicates.

Total definitions (All synonyms, plus supra-ordination)				Total predicates (Classes I-11 and I-14)			
Group A		Group B		Group A		Group B	
J.C.A.	54	G.A.B.	26	J.C.A.	6	G.A.B.	8
T.B.	50	H.A.B.	40	T.B.	0	H.A.B.	3
L.S.M.	16	R.C.T.	26	L.S.M.	4	R.C.T.	24
W.L.P.	46	W.A.W.	40	W.L.P.	6	W.A.W.	9
C.B.T.	44	L.L.W.	51	C.B.T.	3	L.L.W.	0
Totals	210		183		19		44
				Mean var.	1.6	Mean var.	6.16

Jung is therefore of no help to us in this experiment. But that there is such a thing as a predicate-type may, I think, be inferred from a glance at R.C.T.'s results! Of these 24, 11 were given on the first trial, 13 on the second, so that the characteristic does not appear to be a temporary variation.

Some of our methods have proved successful, others have not. To establish just how far we have gone towards the formulation of a genuine diagnostic test for literary and scientific types, I classify the ten members of Groups A and B in each respect in which we have thought definite conclusions justifiable as to type differences (except the matter of proper-names), using again the criterion of "typical" vs. "atypical."

	Parts of speech			Meaning-relation								Totals		
	Noun-stim.	Adj.-stim.	Verb-stim.	(a)	(c)	(d)	(e)	(f)	(h)	Repetitions[31]	Av. of community[31]	t	a	—
J.C.A.	–	a	a	a	t	t	t	a	a	t	t	5	5	1
T.B.......	t	t	–	t	–	t	t	t	t	t	t	9	0	2
L.S.M....	t	t	t	t	t	t	t	t	t	t	t	11	0	0
W.L.P....	–	t	t	–	a	t	t	t	t	t	t	8	1	2
C.B.T....	t	a	t	t	t	a	t	t	–	a	a	6	4	1
G.A.B....	t	a	t	a	a	a	t	t	a	t	t	6	5	0
H.A.B....	a	t	t	t	t	t	t	–	t	t	t	9	1	1
R.C.T....	t	t	a	t	a	–	t	t	t	a	a	6	4	1
W.A.W. .	t	a	t	t	t	t	t	t	–	t	t	9	1	1
L.L.W....	a	t	t	t	t	t	t	t	t	t	t	10	1	0
Totals for *typical* and *atypical*........................												79	22	9

This makes J.C.A. undecided and leaves G.A.B. just over the edge on the typical side. We have nine typical subjects and one undecided. I think we have made some progress towards a diagnostic test.[32] We could improve this by leaving out the second column, which has four atypical subjects, only one other column having as many as three. We get the following scores:

	t	a	–		t	a	–
J.C.A.	5	4	1	G.A.B.....	6	4	0
T.B.......	8	0	2	H.A.B.....	8	1	1
L.S.M.....	10	0	0	R.C.T.....	5	4	1
W.L.P.....	7	1	2	W.A.W....	9	0	1
C.B.T.....	6	3	1	L.L.W. ...	9	1	0

These ten criteria therefore give *every* subject as typical. Of course, to make sure that we have a real criterion we ought to proceed to apply this to a number of other subjects. Because of the length of time this would involve, it has not yet been undertaken.

In closing, I give a summary of what seem to me the out-

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[31] In the results from these two methods the group averages are much nearer together than those obtained by other methods. For this reason absolute variation on either side of the geometric mean, without the use of a margin, has determined whether a subject is "typical" or "atypical."

[32] A good diagnostic procedure would be to try a subject in (e) and (f) of meaning-relations (see page 255), and if these results did not agree as to his status, try the other criteria.

standing positive results of the experiment. Where the results are not decided enough to make conclusions absolutely *sure,* yet seem to make the correctness of the conclusions *very probable,* I have added a question-mark in parentheses.

(1) Scientific subjects tend to give more repetitions than literary subjects. (?)

(2) Scientific subjects tend to greater "community." (?)

(3) Literary subjects tend to give more proper names of persons.

(4) When noun-stimuli are given, scientific subjects give more adjective-reactions.

(5) When adjective-stimuli are given, literary subjects give more noun-reactions. (?)

(6) When verb-stimuli are given, scientific subjects give more verb-reactions, fewer noun-reactions, than literary subjects.

(7) Literary subjects give more associations by contiguity.[33]

(8) Scientific subjects give more opposites.

(9) Scientific subjects give many more "members of a common pair associated by similarity;" and this appears to be a good diagnostic character for a test of subjects.

I believe, however, that the classification of associations which has been worked out for the purposes of this experiment is of more value than any results here obtained; and any claims this experiment has to being considered worth while rest not so much on the conclusions as to literary and scientific types as on the *instrument for future work* which it has suggested,—the new classification of associations, which has been at once the most arduous and the most interesting phase of the experiment.

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[33] In sense defined on p. (248), foot-note 12.

PROFESSOR STUMPF'S AFFECTIVE PSYCHOLOGY

By E. B. Titchener

Professor Stumpf has set forth his psychology of the Feelings in two articles, *Ueber den Begriff der Gemüthsbewegungen* (1899)[1] and *Ueber Gefühlsempfindungen* (1907).[2] We shall be concerned here with the theory of 'algedonic sensations,' and more especially with Stumpf's recent defence of it;[3] but we begin with a brief survey of his general position.

Two remarks may be made by way of preface. It is characteristic of Stumpf's writing, first, that he is not content merely to expound his views, but must also justify them as he proceeds; and this trait, admirable in itself, commits him at times all too whole-heartedly to argument and historical reference and polemical counter-argument. The exposition then comes off only at second best, and questions that press upon the reader are left without answer. It is further characteristic of these articles that they devote a good deal of space to terminology. That again, in the present state of affective psychology, is natural and even necessary: the point to be made is, however, that Stumpf inclines to rely upon context for his own meaning, and is a little impatient with his opponents if they (as they sometimes must) take a similar course.

Our task, therefore, is not always easy, and our right understanding of Stumpf's doctrine is not always assured. In the large, nevertheless, the doctrine is clear enough. Emotions are acts or functions; the sensory feelings or elementary feelings are phenomena, are in fact sensations, and have no claim to the name of feeling. In Brentano's phraseology, emotions or feelings proper are psychical, and sensory feelings or algedonic sensations are physical phenomena. Let us see how this position is worked out.

I. Definition of Emotion

A logical definition regards the class to be defined as a species, and names the proximate genus and the difference. What, then, is the *differentia* of emotion? It differs, Stumpf says, from the sensory feeling of agreeableness and disagreeableness in that it is invariably based upon a judgment: the term judgment is taken very widely, to include even "the very first beginnings of an apprehension and interpretation of impressions of sense." It differs from desire as passive from active; that is to say, it is directed always upon some matter-of-fact, present, past or future, while desire is directed upon some ought-to-be. We may accordingly define it as "a passive state-of-

[1] *Zeits.*, xxi., 47 ff. (referred to in this paper as *BG*).

[2] *Ib.*, xliv., 1 ff. (referred to as *GE*).

[3] *Apologie der Gefühlsempfindungen, ib.*, lxxv., 1916, 1 ff. (referred to as *A*). The monograph *Erscheinungen und psychische Funktionen* (1907) will be referred to as *EF*.

feeling which bears upon a judged matter-of-fact."[4] The factor of judgment is immanent, belongs to the substance of emotion; but the emotion proper is the feeling based on the judgment, and its qualitative peculiarity depends in the first instance upon this immanent psychical basis.[5] The qualitative peculiarity itself, the intimate nature of the emotive feeling (*Affectgefühl*), cannot be defined either in general or in the special case; it can only be experienced.[6]

And the genus? The genus of emotion is, obviously, feeling; but it is noteworthy that Stumpf takes feeling entirely for granted. Although it is of interest to know "what characters unite into a group the enormous variety of these [emotive] states and distinguish them from the other psychical states," he occupies himself wholly with their discrimination from "the other feelings," from sense-feeling, mood, desire and passion.[7] The common character of emotions in this context is that the underlying judgment is passed upon some matter-of-fact; but, of course, there are plenty of such judgments that do not serve as basis of emotion. Even if the matter-of-fact always relates to "one's own or another's life" (and Stumpf does not positively assert this[8]), there are again plenty of *such* judgments that do not serve as basis of emotion. In any case: granted that the nature of the underlying judgment might by some means be specified and distinguished, and granted that judgment is an immanent factor in emotion, still we have the right to ask how feeling, the emotive genus, differs from sensation, from idea, from judgment itself; how Stumpf decides whether the psychical state before him is or is not a feeling; what it is (in this first paper) that justifies him in bracketing together sense-feeling, mood, emotion, desire and passion. The question, apparently, did not occur to him; he names his genus, and then goes in search, within the genus, of his emotive difference.

The later papers take us a little further. We read of a "peculiar interweaving of intellectual with emotional functions;"[9] and though the metaphor is mechanical, and tells us nothing of the actual process, it still helps us to understand Stumpf's statement that the qualitative peculiarity of emotive feeling depends primarily upon the immanent judgment; we gather that the two functions cooperate in some uniform way that parallels, perhaps, the blending or fusion of sensations. We read, again, that emotions as contrasted with sensations are not spatial, not revivable in image, not transferable by association; and, more generally, that "no predicate of the phenomenal world (unless it be time) attaches to the psychical functions."[10] The negatives are better than nothing; but we should like to know the emotive *Merkmale eigener Art,* the characters that correspond with the distinctness of perception, the evidence of judgment, the degree of generality of concept. On the positive side emotion is once described (in a particular context) as "a certain affective attitude (*gemütliche Stellungnahme*) which we term acceptance or rejection, or in its later developments search and avoidance, etc. (Brentano's 'love' and 'hate')."[11] Since judgment also is acceptance or rejection, the emphasis is evidently on the adjective *gemütlich.* We find nothing more, except that emotions may be positive and negative (*Freude-Trauer, Lust-Unlust*);[12] but

[4] *BG,* 48 f., 51, 56.
[5] *BG,* 58, 96 f.; *EF,* 38.
[6] *BG,* 57. [7] *BG,* 48. [8] *BG,* 54. [9] *EF,* 11.
[10] *GE,* 14, 25 *note,* 36; *EF,* 11.
[11] *GE,* 15. [12] *BG,* 89; *EF,* 26; *GE,* 17.

judgments too are affirmative and negative. We are, plainly, not taken very far.

The "psychological nucleus" of emotion is at any rate a *gemütliche Stellungnahme* or feeling proper[13] based upon an immanent judgment of matter-of-fact. In actual experience, however, and from the purely descriptive standpoint, our emotions are complicated, very variously and in very different degree, by organic sensations, muscular sensations, algedonic sensations. "The tone, the color, the temperature of the emotion is undoubtedly conditioned in part upon these sensations;" "the whole particolored variety of this [the emotive] group depends on the cooperation of organic sensations."[14] A fact which does not make for clearness of exposition! For if Stumpf can complain of certain opponents that the difference of *degree* which is all the difference they recognize between sense-feeling and emotion allows them to play fast and loose with illustrations, they can in turn complain of him that the distinction of *kind* which he draws between emotion and algedonic sensation does not prevent his appealing to an emotive *Gesamtzustand* for instances of sensory feeling. The difficulty, indeed, is more than expository; it is observational. Stumpf remarks of a particular experience: "I used to regard it as a purely abstract comprehension, or again as an act of approval or disapproval or the like; but I am now disposed to believe that a sensory agreeableness or disagreeableness is involved." Here Stumpf himself hesitates to decide whether a given experience is all function or is partly phenomenon; and yet "the difference [between phenomenon and psychical function] is the cleanest-cut we know."[15] One can hardly resist the impression, in spite of Stumpf's denial, that affective act and algedonic sensation are somehow *like* each other, and that there is a qualitative resemblance between *Lust* and *Lustempfindung*, between *Unlust* and *sinnliche Unannehmlichkeit*.[16]

After all, then, it is only in the large that Stumpf's doctrine is clear. If we ask for the distinctive character of feeling, we find the adjective *gemütlich* and the remark that the old and convenient distinction of emotional and intellectual functions will be used without prejudice in regard to its definitive accuracy.[17] If we ask for the laws which govern the interplay of the two functions in emotion, we find the phrase *eigentümliche Verflechtung*. If we ask for the method whereby we may discover and identify function and phenomenon, we find that the two are as different as possible, that they are intermixed in our experience, and that Stumpf has sometimes confused them.—It is difficult to believe that those who have held out against Brentano will be convinced by Stumpf's argument.

II. Algedonic Sensations

Stumpf classes together, as algedonic sensations, "first, the purely bodily pains, whether they are set up from within or from without the organism; secondly, the feeling of bodily well-being in its more gen-

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13 *GE*, 17. 14 *BG*, 93 f.; *GE*, 7 f.; *EF*, 27, 37 f.; *A*, 9, 35 f.

15 *GE*, 37; *EF*, 11.

16 *BG*, 57, speaks of the *eigenthümliche Qualität eines bestimmten Affects*. Yet quality is a predicate of phenomena; and according to *EF*, 11, no phenomenal predicate, not even intensity, attaches to functions; time (*EF*, 4, 11) is the one possible exception to the rule.

17 *EF*, 5.

eral and in its more special forms, the latter including the pleasure-component in tickle. the feeling produced by itch, and the sexual feelings; and lastly the agreeableness and disagreeableness that may be connected, in the most various degrees of gradation, with the sensations of all or nearly all the 'special' senses, with temperatures, odors, tastes, tones, colors."[18] Tickle and itch seem here to be dual experiences, composed of a specific cutaneous quality together with a pleasure-quality; but in a later passage tickle-sensations, itch-sensations and lust-sensations are given without qualification as instances of pleasure-sensation.[19]—Some of these sensations (cutaneous pain, tickle, pains resulting from excessive sensory stimuli) are evoked by stimulation of peripheral organs, though even so they seem to be characteristically dependent upon central processes. Others (the 'affective tones' of moderately intensive sense-impressions) are due to central coexcitation, though they on their side owe something to secondary effects at the periphery.[20]

The algedonic sensations thus fall into two groups. The list embraces, on the one hand, cutaneous and organic pains and cutaneous and organic (vegetative) pleasures; and, on the other, the 'affective tones' of the sensations of special sense. The former are peripherally, the latter are centrally initiated. Stumpf naturally tries, by means of the qualifying clauses, to bring the groups into relation: there is little at first sight to connect the cutaneous pain which in normal subjects may be aroused, as an isolated sense-quality, by stimulation of a pain-spot, with the 'sensory disagreeableness' of an odor, which is essentially a 'concomitant sensation of central origin.' The attempt, however, can hardly be judged successful. For the influence of central processes upon members of the first group is limited, if Stumpf's examples are typical, to a change of intensity or degree (analgesia, hypalgesia, hyperalgesia), while the peripheral *Nebenwirkungen* which affect members of the second group are left altogether indeterminate. The class of algedonic sensations is, in fact, held together simply by a bond of function, by the intimate and complex relations which the sensations so named sustain to the feelings proper.[21] If that bond is disregarded, the class falls apart into a group of isolable qualities, which are peripherally excited, and a group of inseparable (or at least not demonstrably separable) qualities, which are centrally coexcited. The line of cleavage may be somewhat blurred by qualifying clauses, but the cleavage is there. It is clear that Stumpf has introduced into psychology "an unique and hitherto unknown class of sensations."[22]

The word 'class' is, to be sure, ambiguous. We might, for instance, speak of a class of temporal sensations, which should include hearing and kinaesthesis; we might, as Hamilton came near doing and Brentano has actually done, speak of a class of sensations with emotional character, as opposed to the class of indifferent sensations;[23] we might speak of the class of concomitant sensations. Stumpf, however, uses 'class' in the meaning of sense-department. He meets the charge of novelty and uniqueness by a reference to the late discovery of muscular sensations; he points out that the algedonic sense (*Gefühlssinn*) by

[18] *GE*, 1 f. [19] *GE*, 22. [20] *GE*, 22, 29; *A*, 1, 23 f.

[21] *GE*, 15. [22] *A*, 3.

[23] W. Hamilton, *Lectures on Metaphysics*, ii., 1859, 494; F. Brentano, *Untersuchungen zur Sinnespsychologie*, 1907, 119 f.

its duality of principal qualities resembles the sense of temperature.[24] Surely he must see that there is a difference, and that the inclusion within a single sense-department of two heterogeneous groups of elements is *prima facie* a ground of objection to his theory? Not, of course, a decisive ground; we must follow where the facts of observation lead us; but still a *prima facie* ground. No one objects to concomitant sensations.[25] The strange thing is that concomitant sensations should, within a certain modality, be ranked alongside of independent and peripherally excited sensations as content-processes of equal rank and title, constitutive of the sense-department.

III. The Critics

Stumpf's *apologia* is in the main a reply to four critics, Brentano, Kuelpe, Titchener and Ziehen. We shall notice only such points of the controversy as seem to be of general significance for affective psychology.

Brentano[26]

Brentano's criticism is little more than a restatement of his own position,[27] and Stumpf justly observes that a counter-statement, however authoritative, is not a rebuttal. The outsider, nevertheless, will take a critical interest in Brentano's views. For Brentano affirms that sensory pleasure and sensory pain are not sense-qualities but affects, emotions, feelings in the true sense, of one piece with intellectual pleasure and displeasure. "For Stumpf they are physical, for me they are psychical phenomena." We thus have a flat disagreement between two representatives of a functional psychology of Brentano's type. Yet the difference between phenomenon and psychical function is, for Stumpf, the cleanest-cut we know; not a single predicate of the phenomenal world (unless it be time) attaches to the psychical functions; and no functional predicate can be assigned to phenomena! If Stumpf himself may confuse (as he admits he may) the phenomenal and functional components in a given experience, and if Brentano can claim as functional what Stumpf is at great pains to prove phenomenal, the inference seems plain that there is something wrong with method.

Brentano gives his method and his criterion: the emotive nature of sensory pleasure and sensory pain is guaranteed by the evidence of

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[24] *A*, 3, 32; *GE*, 22.

[25] *A*, 28 ff. The non-pathological concomitant sensations which Stumpf names (subjective combinational tones, tickle in the nose, after-images, contrast-sensations, the qualities that fill in the blind spot, etc.) are all sensations which we know independently as well as in concomitance. The synaesthesias appear to form a class apart. The photisms of colored hearing, e. g., may show more than one color, may even show visually incompatible colors, at the same place of the (auditory) field, and in certain of their attributes are more akin to auditory than to visual sensations (E. Bleuler, *Zur Theorie der Sekundärempfindungen, Zeits.*, lxv., 1913, 7, 8, 10, 21). They present no analogy, so far as our knowledge of them goes, to the algedonic sensations. The status of the 'musical quality' of tones—Stumpf's last example—is as yet uncertain.

[26] Brentano, *op. cit.*, 121 ff.; *A*, 4 ff.

[27] With the addition remarked in Note 23 above.

introspection.[28] Stumpf rejoins that the guarantee is derived, not from psychology, but from theory of knowledge. Many of us will agree; and still the reply is not satisfactory. For it is to be remembered that this much of 'theory of knowledge' is made by Brentano an integral part of his empirical psychology, and that, if Brentano's book is not truly an empirical psychology, then neither is a great part of Stumpf's own monograph on *Erscheinungen und psychische Funktionen.* The outsider, indeed, will be tempted to say summarily that he does not see how a functional psychology of the Brentano stripe can be written without a theory of knowledge; and, if he does not as psychologist subscribe to the distinction of act and content, will attribute the differences between Stumpf and Brentano to the commingling of logical and scientific method.

Kuelpe[29]

Kuelpe maintains, against sensationalistic theories in general, that the feelings have no sense-organs and leave behind them no "ideational residua or tendency to ideation;" and, against Stumpf's theory of central sensory coexcitation in particular, that all known concomitant sensations have sense-organs, that they stand in unequivocal associative relation to their excitants, and that they are reproduced. Experiments made by a Method of Favorable Opportunities (4 different series, 7 observers, over 240 observations) lead him to the conclusion that feelings can in no case be imaged or reproduced; "die Vorstellbarkeit fehlt." Four observers gave no report of affective image; one showed "a certain doubt in a few unusually difficult cases," but inclined on the whole to deny the image; two (one of whom could not complete the experiments) occasionally reported images, or experiences that might be interpreted as imaginal, but all such reports were inconclusive.

Stumpf replies that some persons cannot image tones or odors. The force of this reply depends upon the meaning of certain sentences in Kuelpe's paper. He does not appear to say that everybody can image every kind of sensation; his position seems to be that every kind of sensation can be imaged by somebody. There is, however, 'a certain doubt.' Stumpf urges, further, that Kuelpe has mixed together sensory feelings (algedonic sensations) and emotions. The experiment thus involves a *petitio principii;* for algedonic sensations might be reproducible, and emotions not. We may rejoin, on Kuelpe's behalf, first, that the four experimental series dealt separately with affectively toned sensations, with strongly affective situations, with emotions, and with empathy; and secondly that the few doubtful cases which are described in detail are drawn from the third and fourth series as well as from the first. Had Stumpf's algedonic sensations (the affective tones of the first series) shown any preponderant tendency to leave images, that fact would assuredly have been noted.

All in all, Stumpf takes Kuelpe's experiments more lightly than they deserve. They are, indeed, too briefly reported, and they are by no

[28] Both terms are technical! See F. Brentano, *Psychologie vom empirischen Standpunkte,* i., 1874, 35, 119.

[29] O. Kuelpe, *Ein Beitrag zur Gefühlslehre,* in *Bericht über den III. internationalen Kongress für Philosophie,* 1909, 546 ff., 554 f.; *Zur Psychologie der Gefühle,* in *VIme Congrès international de psychologie,* 1910, 183 ff., 224 ff.; *A,* 10 f.

manner of means final. On the positive side, however, they were carefully planned, they covered a wide field, and they were performed 'without knowledge' by the observers. As our empirical evidence stands at present, no discussion of affective theory can afford to dismiss them without serious consideration.[30]

Titchener[31]

Stumpf thinks that I should accept his theory of central sensory coexcitation were it not for my own view that affections lack the attribute of clearness.[32] The mistake is natural, but is none the less a mistake. If my present view is wrong, and if the affective processes are not to be classed apart from sensations, then I believe that they must reduce, one and all, to complexes of organic sensations. I cannot yet assure myself that such reduction is possible; but I should, in fact, prefer almost any form of thorough-going sensationalism to the theory which brackets together cutaneous pain (which I find to be sometimes pleasant), and the disagreeableness of asafoetida, as qualities of a single sense.

Stumpf thinks, further, that I am inconsistent in objecting to his physiological hypothesis[33] when I give high praise to G. E. Müller's psychophysical theory of achromatic vision. What I wrote was this: "My objection is not by any means to psychophysics as such. I do object, however, to the basing of a psychological argument upon a speculative psychophysics. And we have a peculiar right to object, in the present instance, because Stumpf promised us a descriptive psy-

[30] Stumpf's concluding remark hinges on one of those terminological confusions which almost bring despair of mutual understanding. Kuelpe and all his observers distinguished *Schmerz* from the *Unlust* connected with it. "I cannot resist the impression," says Stumpf in comment, "that at bottom we both mean the same thing." But for Kuelpe pain is a quality of cutaneous sensation, and *Unlust* one of the two qualities of elementary feeling; *Lust und Unlust* are *elementare Bewusstseinsinhalte.* For Stumpf pain is an algedonic sensation, the equivalent of a sensory feeling, and corresponds with Kuelpe's pain *plus Unlust* (cf. the *durchaus komplett* of *GE* 17), while *Unlust* is an emotion, a function, not a content at all (*ib.*). So both Kuelpe and Stumpf distinguish *Schmerz* from *Unlust,* while they mean at bottom radically different things.

[31] *Feeling and Attention,* 1908, esp. 81 ff.; *A,* 11 ff.

[32] Stumpf has criticised this view a little hastily. Thus he argues (1) that not all sensations have the same attributes. True: but my point is that clearness is an essential attribute of sensation. He points out (2) that the epicure wallows in his enjoyment. No doubt: but his mind is occupied, I suppose, with things to eat and drink, not with his present pleasure in eating and drinking. (3) He quotes Johannes Müller to the effect that a sensation of pain becomes the more painful the more exclusively it is made the object of attention. For me too, however, the sensation of pain is thus made clearer; pain is for me a sensation.

[33] The statement that I devote sixteen pages (98-114) to this objection is due to sheer oversight. The objection ends on p. 101; then comes a discussion of affective imagery, pp. 101-104; then a discussion of the separability of algedonic images, pp. 104-111; then there is a return to psychophysics, pp. 112-113 (and this is what misled Stumpf); and finally a brief summary of Stumpf's paper, pp. 113-114.

chology." Müller's work is entitled *Zur Psychophysik der Gesichtsempfindungen.*[34]

Finally, Stumpf accuses me—and makes Marshall his partner in the charge[35]—of vacillation in the use of terms. My usage is, as a matter of fact, definite. I parallel affection, the feeling-element, with sensation; I parallel feeling, a complex of sensory and affective processes in which affection dominates, with perception; I parallel emotion with thought.[36] The instances to the contrary which Stumpf has brought together represent, not my usage, but that of the authorities I quote.[37] I cannot, for instance, translate Stumpf's *Schmerz* and *sinnliche Unannehmlichkeit* by the same English term.

Ziehen[38]

Ziehen's affective theory, as set forth in the *Leitfaden* of 1891, may be summed up as follows. Every sensation has three attributes, quality, intensity, and affective tone. This last is not, however, a necessary attribute; only a limited number of sensations rise above or fall below the point of affective indifference. It is in its nature a qualitative attribute, and is itself intensively variable. Its qualities are two and two only; and since these qualities are added, as feelings of pleasantness and unpleasantness, to sensations and memory-images, it "represents as it were a sixth sense." Pleasantness and unpleasantness differ, nevertheless, from sensations proper. First, they cannot stand alone, though in the case of pain (which is not a specific sensory quality, but the feeling of unpleasantness which accompanies very intensive cutaneous sensations) we may think that we experience affective tone in complete independence of sensation. Secondly, they are coupled with memory-images. Thirdly, they depend upon very general properties of stimulus and receptor, and are correlated with "a reaction of the cerebral cortex to the stimuli which come to it from the outside."[39]

This, the teaching of 1891, is also essentially the teaching of 1914.

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[34] "I am very doubtful," says Stumpf, "whether Müller himself had any notion that his hypothesis was really solving the descriptive problem." If he will reread Müller's § 6 (*Zeits.*, x., 1896, 25-33) I think his doubt may be removed.

[35] Wrongly, as I thought on rereading Marshall; wrongly, as Marshall himself assures me. "I do find difficulties in connection with your terminology," writes Dr. Marshall in a personal letter (Decr. 7, 1916), "but I do not feel that they lead me astray as to your meaning; and certainly in the article referred to I was concerned to complain that you were influenced by sensationalistic ways of thought"—the interpretation I had myself put upon the passage to which Stumpf appeals.

[36] *Feeling and Attention,* 34; *Outline of Psychology,* 1902, 102, 224, 229.

[37] I find one breach of this rule in the lectures from which Stumpf quotes: on p. 49 I speak of general "views of feeling" in the sense of general theories of affective experience. Stumpf does not mention this lapse, and I therefore hope that he was not misled by it.

[38] *Leitfaden der physiologischen Psychologie*, 1914, esp. 222 f.; *Die Grundlagen der Psychologie,* esp. ii., 1915, 202 ff.; *A,* 17 ff., esp. 19, 34.

[39] *Leitfaden,* 1891, 28, 82 f., 84 f., 93 f. I have, for simplicity's sake, made no reference to the spatial and temporal attributes of sensation.

Pleasantness and unpleasantness are now not the sole but the principal or dimensional qualities of affective tone, and as such show varieties of qualitative shading. The identification of pain with unpleasant feeling has become a matter of interpretation; Ziehen still holds to it, on the ground that there is no demonstrative proof to the contrary. Affective tone remains an attribute of sensation, but is no longer coordinate with the other attributes; it is dependent upon them; it resembles them in that it cannot stand alone, but differs in that it may lapse and may vary intensively.—There are no further changes, either here or in the *Grundlagen* of 1915. The newer work comes, indeed, even closer to the exposition of 1891 (to which it expressly refers); for the attribute of affective tone, though it differs in many ways from the other attributes of sensation, is said nevertheless to be "entirely coordinate" with them.[40]

Aside from the misuse of the term attribute—to which Ziehen sticks with the obstinacy of a man who has made a logical slip and will not be convicted of it—this theory is, on the whole, simple and consistent. Under certain conditions, which can be stated in general but have not yet been worked out in detail, the central excitatory process which underlies sensation and image involves a further excitatory process, also central, which is correlated with feeling. An affective quality then blends with the sensory quality of sensation or image. Such a view, Ziehen says, is "diametrically opposed" to the view of Stumpf.

He begins with pain: partly because Stumpf too begins with pain, but partly, no doubt, because the establishment of a pain-sense with specific end-organs would imperil his own theory. Pain for Ziehen is a feeling, and must therefore be centrally originated. We have seen that the positive statement of 1891 becomes a matter of interpretation in 1914; but Ziehen's opinion has not changed; "I maintain," he writes in 1915, "that pain represents nothing more than a quality which accrues to other [to sensory] qualities—contact, warmth, cold—and which is conditioned upon a process centrally superadded." He argues accordingly that even if special pain-spots and special pain-paths are demonstrated, they do not necessarily imply a specific sense-quality of pain; there are ways out of that conclusion. They are, however, by no means proved; still less is it proved that pain-quality may occur independently of cutaneous sense-quality.

All of which, be it said with respect, seems curiously roundabout. For Stumpf, pain is an algedonic sensation, the equivalent of the 'complete sense-feeling" of other psychological systems. It is peripherally excited, but stands (so far as intensity at any rate is concerned) under central influences. For Ziehen, pain is not a sensation of any kind, but a feeling proper, centrally excited. Yet the weight of evidence surely goes to show that pain is a cutaneous sensation, like pressure and warmth and cold, and that it may be (to use Stumpf's terms) either agreeable or disagreeable. Would not both Stumpf's and Ziehen's theories gain in simplicity and consistency if this evidence were accepted? Then the agreeableness or disagreeableness of pain would be, for Stumpf, a centrally coexcited algedonic sensation, for Ziehen a centrally originated feeling. Pain itself, the cutaneous sense-quality, would be out of discussion.

Meanwhile the discussion includes it. And Stumpf sharply rebukes Ziehen for overemphasizing the rôle of pain as algedonic sensation,

[40] *Leitfaden*, 1914, 47 f., 197 f., 200, 204, 220 f., 222, 224, 273 f.; *Grundlagen*, ii., 79, 217, 245.

its peripheral origin and its isolableness. These physiological matters, he declares, are for him secondary; and the group of algedonic sensations that stands in the forefront of his personal interest is not the peripheral group but the group that comes with central coexcitation.[41] We have seen, however, that Ziehen had reasons of his own for disposing of pain at the start, and that he has his own theory as well as Stumpf's in mind when he labors at the peripheral problem. What, now, does he say of the concomitant sensations of central origin? He directly impugns them, and concludes that "in the higher sense-departments Stumpf's theory is not tenable." There are, he declares, two physiological possibilities. First, a sensory excitation may excite associatively the sensory center for pleasure-pain, the central area directly excited from the pain-spots and pleasure-spots of the periphery. In that case, however, we should expect the concomitant sensation of disagreeableness to show a resemblance to pain, and we should expect intensive change of the primary sensation to be paralleled by continuous and similarly directed change of the concomitant sensation. Neither expectation is realized, and this first possibility thus becomes highly improbable. Secondly, the central elements which are the seat of the coexcited algedonic sensations may be out of all connection with the periphery. In that case, however, we cannot account for their specific sensory energy; the unconnected central elements would stand in a class altogether apart; for there is no known sensory quality that depends solely on central excitation. The second possibility, then, is also highly improbable.

Stumpf's reply, summarised, is again that to him as psychologist these neurological considerations are irrelevant. And on the face of things he has the better of his opponent; Ziehen sometimes writes carelessly, and Stumpf is a keen controversialist. Yet one may doubt if he has grasped the real meaning of Ziehen's objections. The gist of the matter, for Ziehen, is that these pleasures and pains, these sensory agreeablenesses and disagreeablenesses, are *not sensations*, whether centrally or peripherally excited, whether independent or concomitant. He argues the issue in his own, predominantly physiological way; but his point throughout is that we know the class of sensations, and that the experiences whose nature is in dispute are, emphatically, not of that known class.[42] When, therefore, Stumpf declares that, had he been better acquainted with Ziehen's position when he wrote the *GE*, he would unhesitatingly have cited it in confirmation of his own view,

[41] "On this point," he writes, "Titchener has understood my doctrine almost more correctly [than Ziehen]; at all events he has understood it in the opposite sense." I am grateful for the praise, but—as my text goes on to show—I fear it is undeserved.

[42] An instance may be useful. In *Leitfaden* 223 Ziehen asks: "How does the concomitant sensation come by its pleasurable or unpleasurable character?" and Stumpf rejoins: "It *is* agreeableness or disagreeableness." That is not a direct answer, because Ziehen is asking how a 'sensation' can come to be a 'feeling.' So in *Grundlagen* ii. 214 Ziehen asks: "Whence do these [central] elements derive their specific sensory energy in the sense of pleasure and pain?" and Stumpf returns: "I put the same question to him, in regard to his own 'affective tone.' The question is too much for either of us." But Ziehen is not asking why pleasure is pleasant and pain painful; he is asking how specific *sensory* qualities can appear under conditions which, by all physiological analogy, are inadequate to their arousal.

the obvious comment is that he would in that case have been stressing the physiological side of Ziehen's theory to the neglect of its psychological import. Conversely, when Ziehen, by a process of elimination, reduces the possibilities of central coexcitation to that which he himself assumes, his conclusion is that Stumpf's theory of algedonic sensations must therefore give way to his own theory of affective tones. Stumpf, aiming at a descriptive psychology, assimilates the two theories on the ground of physiology; Ziehen, writing in terms that are mainly physiological, is concerned to differentiate them on the ground of psychology.

In what sense, however, are the theories "diametrically opposed?" Ziehen's affective tone, a contingent attribute of sensation, has its own quality, intensity, and locality, and leaves an image behind it.[43] Is it so radically different from a concomitant sensation?—It differs by the very fact that it is affective tone, feeling proper, the primary source of emotion and mood and passion. Stumpf "draws the line," as he says, between algedonic sensation and emotion; the one is phenomenon, the other is function; there is no transition, logical or empirical, from sensation to feeling. That is the difference; and that is the reason why Ziehen, though he takes issue with every argument that he ascribes to theorists of a third type, who stand for an elementary affective process coordinate with sensation, still reserves his 'diametrical' opposition for the theory of Stumpf.

IV. The Motivation of Stumpf's Theory

Stumpf appears to have arrived at his theory of algedonic sensation by a threefold path. It appeals to him, in the first place, from the side of logic. We have to choose between the theory of an independent affective element and the theory which identifies sense-feeling with sensation. The latter hypothesis is the simpler, and the scientific principle of economy thus throws the burden of proof upon its rival. Since, however, proof is not forthcoming,—since there is no positive character whereby sensation and simple feeling may be distinguished,—we are logically bound to make trial of sensationalism. That is the logical road to the *Gefühlsempfindungen*, and every theorist likes to have such a road at his disposal. Unfortunately, every theorist has! For Brentano might say that his single class of 'phenomena of love and hate' is simpler than Stumpf's duality of affective function and affective phenomenon; and Ziehen might say that his derivation of the emotions from a root of sense-feeling is simpler than Stumpf's denial of a logical transition from the one to the other. Logic indeed, as Stumpf in a neutral context would probably agree, must follow the facts and not precede them. If the facts are complex, no twist of logic can make them simple.

Secondly, Stumpf's theory appeals to him from the side of what is commonly called systematic psychology. In 1899 the sense-feelings are still feelings, but they are feelings "called forth directly by the sense-impression." There is thus a division in the affective group. On the one hand are the emotive (and kindred) states, which depend upon intellectual or volitional activity, present or past, and on the other hand are the directly stimulable feelings of sense. Stumpf does not base a formal argument upon this divergence,—as if experiences called forth directly by the sense-impression were *ipso facto* phenomena,—

[43] *Grundlagen*, ii., 221, 223.

but there can be no doubt that he is keenly sensible of it, and that his theory of emotion thus leads up to his theory of algedonic sensation. The outcome, for systematic psychology, is simply, as we have seen, that the line of cleavage is transferred from feeling to sensation. Stumpf recognises the new difficulty, and tries (unsuccessfully, as we also saw) to overcome it. Meanwhile his theory of emotion has passed from the stage of hypothesis to that of dogma;[44] the systematic line is drawn once and for all; and the sensations must accordingly make out for themselves the best case they may.

We have, however, not yet touched the heart of the matter. Stumpf's primary motive is his desire, thirdly, to account for the feelings aroused in us by tones. His interest in these feelings is deep and of long standing; and if he now speaks in general terms of the individual development and the generic evolution of the sense-feelings, it is nevertheless to tones that he immediately turns as offering the richest material for investigation, historical, individual, ethnographical. The theory of algedonic sensations enables him to cope with the recorded changes of affective reaction, and especially with those due to habitual direction of attention, to disposition of judgment, to habits of all kinds.[45] He does not go beyond the bare statement, and we can only guess that in his detailed treatment the emphasis would be rather on 'concomitant' than on 'sensation.' Would that—one must ask the question—would that so very much matter? Stumpf could give us a book on *Tongefühle* and *Tongefühlsempfindungen* such as no other psychologist can write. Why will he not turn his back on controversy and write it?

V. The Historical Background

There are not a few writers, Stumpf reminds us, especially among the philosophers of the 17th and 18th centuries, who "draw a sharp line of distinction between the sensory feelings, which have the character of sensation, and the emotions." In so far as this distinction is made by men who are not thorough-going sensationalists, it offers historical support to his own theory.

He first appeals to Descartes, and we grant without demur that Descartes distinguished sense-feeling from emotion. Stumpf's algedonic sensations are included by Descartes, along with hunger, thirst, warmth, cold, etc., among the perceptions that refer to our own bodies. They differ, accordingly, both from perceptions like color and sound, which refer to objects outside us, and from emotions; they are what we should today call organic sensations. In the large, then, Descartes agrees with Stumpf. When, however, we turn to the emotions, we find in Descartes a theory which is closely akin to that of James. Not only is the first draft of the theory like James' first statement, but the modification which it undergoes in the course of its working-out also anticipates the changes in James' later attitude. So far is Des-

[44] *GE,* 7, note 2.

[45] *GE,* 42, 44 ff.; *A,* 31. The tests of the bandsman N. (*Verlust der Gefühlsempfindungen im Tongebiete* (*musikalische Anhedonie*), *Zeits.,* lxxv., 1916, 39 ff.) lead only to the conclusion that "the 'affective tone' is separable from the tones themselves, that is to say, is not an immanent attribute." Neither is it an immanent attribute for Brentano, for Kuelpe, for Ziehen, or for myself! The report is interesting, on various grounds, but it leaves the algedonic theory where it was.

cartes even from gross agreement with Stumpf that he here represents a view which Stumpf expressly rejects.[46]

Malebranche, again, counts the sensations of pleasure and pain in the same list with sights and sounds, odors and tastes, and in a passage to which Stumpf refers contrasts the sensation of pain with the sorrow it produces. The ground of this "essential difference between sorrow and pain" lies, curiously enough, in the observation that sorrow "is always pleasant to it self," "is ever agreeable, when there's occasion to be mov'd by it." The theory of emotion which Malebranche represents is, for the rest, as Lange himself points out, an anticipation of the vasomotor theory.[47]

Hume regards the bodily pleasures and pains as impressions of sensation, and the passions as impressions of reflexion. So far he supports Stumpf. But "hunger, lust, and a few other bodily appetites" are not sensations but passions, arising from "a natural impulse or instinct." There is, moreover, "an original instinct" whereby the mind tends to hold fast to pleasure and to avoid pain, so that "in order to produce an affection of any kind, 'tis only requisite to present some good or evil." That is not Stumpf's doctrine.[48]

We come next to Kant, whose distinction of the *sinnlich angenehm* from the *wohlgefällig* is, as Stumpf says, well known. Yet Kant's psychology is a psychology of faculties, and Kant is the first who explicitly ranges the faculty of feeling alongside those of cognition and desire.[49] The *Anthropologie* instructs us, in detail, that the *Gefühl der Lust und Unlust* is not to be classed with the vital sensations; these sensations are related to emotion, but are not themselves emotions.[50] Emotion, on the other hand, is a feeling, and ought to be discussed under that heading, though for convenience it will be postponed to the section which treats of the faculty of desire.[51] Finally, "we pass judgment upon pleasure and pain [*Vergnügen und Schmerz,* which are *Gefühle der Lust und Unlust*] by a higher satisfaction or dissatisfaction [*Wohlgefallen oder Missfallen*] with ourselves, namely the moral." So that Kant marks off the sense-feelings from sensations, groups them with the emotions, and draws in moral terms the distinction which Stumpf as psychologist approves. Kant's phrases, however, are psychologically significant: for dissatisfaction with a pleasure is called a 'bitter joy,' and satisfaction with a pain is called a 'sweet pain.'[52]

There remains Johannes Müller, who enumerates as the 'modes' or 'energies' of the *Gefühlssinn* pressure and contact, tickle and itch, lust, pain, warmth and cold.[53] The list has a very modern look, and there is no mention of *Lust und Unlust.* These are, in fact, said to be "presented states of conation" (*vorgestellte Strebungszustände*).[54]

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[46] *A,* 3; D. Irons, *Descartes and Modern Theories of Emotion, Phil. Rev.,* iv., 1895, 291 ff.

[47] Cf. my *Text-book of Psychology,* 1910, 479; N. Malebranche, *Treatise concerning the Search after Truth,* 1700, 104 f.

[48] D. Hume, *A Treatise of Human Nature,* ed. L. A. Selby-Bigge, 1888, 438 f.

[49] W. Volkmann von Volkmar, *Lehrbuch der Psychologie,* ii., 1885, 302.

[50] I. Kant, *Anthropologie in pragmatischer Hinsicht,* 1798, 46 f., 223.

[51] *Ib.,* 176. [52] *Ib.,* 179 f.

[53] *Handbuch der Physiologie des Menschen,* ii., 1840, 249, 497 f.

[54] *Ib.,* 537.

"All passions," we are told, "may be reduced to *Lust, Unlust* and desire, and all alike contain as elements: presentation of the self or of the individual life, presentation of the magnitudes opposed to the individual life, inhibiting or furthering it, self-preservative conation, and inhibition or furtherance of that."[55] Here is a theory of emotion which differs from Stumpf's in that a reference to self is made an integral part of every emotive state.[56] And if Müller had held to his terminology, everything would have been in order. As a matter of fact, he does not. Conation is inhibited, in the simplest case, by pain or disagreeable bodily sensation; it is furthered, in the simplest case, by bodily sensation of well-being and pleasure (*Lust*).[57] The adjectives 'agreeable' and 'disagreeable' are thus used both of feelings and of sensations; pleasure is both a state of conation and a bodily sensation; well-being is both a bodily sensation and an emotion.[58] We must accept the formal list of sensations and the formal definition of *Lust und Unlust,* and we may suppose that Müller, having thus safeguarded himself, is at no further pains to avoid the looseness of expression current in his day;[59] but even so it would seem that he finds a qualitative likeness between *Schmerz-Wollust* and *Unlust-Lust.*[60] How all this can bring comfort to Stumpf, it is difficult to see.

Cold comfort, indeed, is all that can be expected from the appeal to history. For every systematist will make a difference between sense-feeling (or algedonic sensation) and emotion.[61] But if the line is drawn for reasons of philosophy, then the distinction has no relevance for psychology; and if it is drawn for reasons of psychology, then all of those reasons must be taken into account and given their due weight; we may not stress a resemblance here and slur a difference there. Stumpf's doctrine is that a separate sense-department furnishes algedonic sensations, for the most part concomitant and centrally excited, whose dimensional qualities are pleasure and pain. Algedonic sensations and emotions are heterogeneous; there is no logical connection between them, nor is it possible to pass empirically, by intermediate processes, from the one to the other; Hume's "easy transition from pleasure to love" involves a leap from phenomenon to function. For emotions are functions, feelings based on immanent judg-

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[55] *Ib.,* 539. Müller uses 'passion' as the equivalent of *Gemüthsbewegung* in general; *ib.,* 525, 539.

[56] *Ib.,* 538: "Das Selbstgefühl ist ein Element aller Leidenschaften." Another difference is that Müller regards aesthetic 'feeling' as purely intellectual: *ib.,* 537.

[57] *Ib.,* 540 f.

[58] *Ib.,* 258, 537; 537, 541; 540, 541.

[59] It is noteworthy that in this discussion he nowhere employs the terms *Schmerzgefühl* and *Unlustempfindung.* Even in the prolegomena to the book On the Senses, where *Gefühl* is the name of a sense-department, *Schmerz* is only very occasionally called a *Gefühl* (259, 263); the rule throughout is *Schmerzempfindung.* The phrase *Empfindung der Unlust,* on the other hand, occurs (so far as I have read) only once, in the discussion of the temperaments (578), and then on a page which has already made *Unlust* a *Gemüthsbewegung!* As a rule the bracketing of *Lust, Unlust* and *Begierde* (*Begehrung*) is strictly adhered to.

[60] *Ib.,* 258, and *Phant. Gesichtserscheinungen,* 1826, 88 (a passage quoted by Stumpf).

[61] *A,* 36.

ments, and functions and phenomena have nothing whatsoever in common unless it be time. A sensationalistic theory of emotion is therefore a psychological blunder. Functions and phenomena are mingled in our experience, and relations between them are directly given, but the task of the psychologist is, precisely, to analyse what is empirically linked and blended.[62]

If, now, we are looking for historical antecedents of this doctrine, we shall surely have in mind central sensory concomitance, immanence of judgment, mutual exclusiveness of predicates. We find, however, that emotion may be distinguished from algedonic sensation, and yet may itself be explained in James' or Lange's way; that higher and lower feelings may be distinguished, and yet may both alike be feelings proper; that algedonic sensation may be distinguished from emotion, and yet may bear a qualitative likeness to emotion. Stumpf's historical background turns out to be a dissolving view.

Conclusion

The outcome of this review is that Stumpf's *apología* has not improved his position. He seeks to persuade us by argument rather than by exhibition of facts. If, however, we are to judge by logic, then Brentano, with whom Stumpf naturally compares himself, still maintains the advantage. Suppose that I find an odor disagreeable. According to Brentano, I experience the act of hating the act of sensing the physical odor; I have a true emotion; to find a thing disagreeable is as much as to dislike it. According to Stumpf, I experience in the simplest case the act of remarking the concomitant sensation within a phenomenal blend composed of odor and concomitant disagreeableness; whereas, if I dislike the odor, I experience an interweaving of the acts of judging and hating the concomitant sensation within the phenomenal blend. The real strength of Stumpf's doctrine lies, not in any claim to logical simplicity, but rather in its surrender of logical simplicity at the command of facts, namely, of its author's observations of tonal feeling. We have found, in the foregoing pages, that psychologically the algedonic sense is unlike any known sense-department, and that physiologically the status of the centrally excited concomitant sensations is precarious. The algedonic sensations are, nevertheless, the result of Stumpf's study of tonal feeling, just as his doctrine of fusion is the result of his study of tonal sensation: only in the one case he has given us his empirical evidence, while in the other he tantalises us by generalities. In science, however, facts are always stronger than arguments, and the doctrine of fusion itself has won its way by detailed and reiterated appeal to fact. If Stumpf's affective psychology is to make converts, we must have the fourth volume of the *Tonpsychologie*.

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[62] *A*, 35; *EF*, 4 f., 6 f.

NOTES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

I. A Compressed Air System for Demonstrational Purposes

By E. G. Boring and W. S. Foster

Gasometers like the Whipple tanks are unsatisfactory sources of compressed air for acoustical apparatus even in lecture-demonstrations. Some laboratories which are equipped for research purposes with a blower-system of compressed air provide an outlet at the lecture-desk. For laboratories which lack such equipment or which, like the Cornell Laboratory, cannot connect the main system with the large lecture-room, the centrifugal type of vacuum-cleaner will be found to constitute a very satisfactory source of compressed air for lecture-demonstrations involving blown bottles, variators, pipes, reed-boxes, *etc.*

In the Cornell Demonstrational Laboratory we have installed the motor and blower of the Frantz Premier vacuum-cleaner. In this machine the fan is connected directly to the motor-armature and operates inside an aluminum casting, to which connection is made by a special "blower attachment." The list-price of the complete machine is $35.00, but the manufactures (Frantz Premier Co., Cleveland, O.) were kind enough to supply us with the motor, fan, casting, and blower attachment at a considerable reduction.

The motor is suited for either A.C. or D.C., 110 volts. When in operation it makes a good deal of noise. The whole outfit, however, is small; and the noise was muffled by placing the motor in a double-walled box with sawdust between the walls. The outside dimensions of the box are 16 by 16 by 8 inches; the thickness of the walls, 2 to 3 inches. The noise was still further reduced by placing the box in the Demonstrational Laboratory and leading the air by a 1-in. iron pipe to the lecture-desk in the adjoining room. To prevent conduction of vibration along this pipe a short piece of rubber tubing was used as a coupling between pipe and blower attachment. A gate-valve at the lecture-desk regulates the air-pressure, and a push-button behind the desk controls the motor. Beyond the gate-valve a removable set of distributing connections is attached. At the one side of a full-sized T a second T with a separate valve makes possible the combined or independent use of two tonometers, while at the other side of the T seven ⅜-in. gas-cocks, tapped into the 1-in. pipe, supply outlets for blowing variators, bottles, pipes, and whistles.

With this arrangement the pressure maintained is sufficient to operate simultaneously at full intensity almost half the reeds of the large Appunn tonometer (512-1024 vs. by 4 vs. steps). The Galton whistle, piston-whistles, bottles, and variators are easily blown either separately or in combination when connected with the gas-cocks. The tone produced is sensibly constant. Even the addition of two or three variators (which draw a relatively large volume of air) does not materially alter the intensity of the Galton whistle, for which a fairly high pressure is necessary.

We have found that the demonstrations are most durably set up if flexible metal tubing is used to connect the gas-cocks with the smaller apparatus, and if short straight pieces of large-bore rubber tubing connect the large openings of the iron pipe with the tonometers. Bends in the rubber tubes leading to the tonometers are avoided by permanent elbows fixed to the vertical intake-openings. A large 1½-in. pipe-flange, bearing a 1½—1-in. reducing ell and a nipple, has been screwed to the bottom of each tonometer over the inlet opening; the length of rubber tubing can thus be slipped directly over the horizontal nipple without bending.

II. Delboeuf Disks and the Kirschmann Photometer

By E. G. Boring

Of the three methods described by Titchener[1] for the experiment on the application of the Method of Equal Sense Distances to brightnesses, the first, as Titchener points out, is mechanically unsatisfactory, and the third, which requires three motors or a triple color-mixer, is often impracticable. The Delboeuf disks of the second method are made by passing black sectors on white cardboard. It is difficult to cut these sectors exactly, and still more difficult to paste them correctly, as they must be perfectly centered and separated by 180° with an error of less than half a degree. If the space order is to be varied, two disks have to be cut. The movable sector can be made to use with both space orders, but must in that case be notched in a way that makes it easily breakable. Both cardboard and black paper are often smudged in pasting. With use the pasted black paper is likely to get shiny, the cardboard dirty, and the sector broken. An accident to any one of these three pieces means the remaking of all three, if the tone of the black and the white is to remain the same. Even when the experiment proper has been successful, it sometimes happens that the student gets the black sample pasted on the Kirschmann photometer in such a way that it does not exactly resemble the black of the disks. In general we have found that every pair of students requires a new set of disks, which take a couple of hours to prepare.

We have therefore substituted in the Cornell Laboratory the method described by Martin[2] for the contrast experiment. We cut disks of three sizes from each of the two gray papers which have been selected as the terminal stimuli. The middle-sized disks are slotted and fitted together; when mounted concentrically on a mixer, with a small disk of the one gray and a large disk of the other, they constitute the variable ring. Space order is reversed by substitution of the other (large and small) disks. The method saves the time required for cutting on the disk-cutter arcs of limited length, for laying out radii at exact angles, and for pasting the paper on the disks; and it avoids the errors arising from poor spacing and centering, and from the frequently non-uniform appearance of a pasted black paper. Moreover, the use of gray papers instead of black and white makes an accurate adjustment of the variable easier; for the total range of

[1] E. B. Titchener, *Experimental Psychology*, II, i, 87-90.
[2] L. J. Martin, *Amer. Jour. Psychol.*, 24, 1913, 33f.

variation is now 360° instead of a comparatively small sector (*e. g.*, 30° in the disk figured by Titchener).

All the objections urged against the Delboeuf disks apply with equal force to the Kirschmann photometer.[3] The photometers are difficult to make, and soon get dirty. The samples of paper to be tested, even when accurately cut, are difficult to center and may be smudged in pasting. We have accordingly applied Martin's arrangement to the photometer. If the motor is rapid, it is not necessary to cut two sector openings in the disk; one opening of 180° will not flicker. Thus we cut in a white cardboard disk (diam. 26 cm.) one 180° sector of an annular ring of radii 9 and 6.5 cm. (The arc of the smaller radius need not be cut carefully, since it lies beneath the next disks.) A disk of the paper to be tested, coupled with a disk of the white cardboard (diam. 14 cm.), forms the concentric variable ring which lies inside the black (hole) and white (cardboard) ring. A small white cardboard disk (diam. 10 cm.) fills the center. These disks can all be cut on the disk-cutter with so little handling that they are not likely to get dirty before use. No pasting is required, and centering is exact. But one incomplete arc (instead of five) has to be cut. The fine adjustment can be added, as usual, if it is desired. For very light grays it is well to have a large disk with a 90° annular sector instead of the 180° sector.

It is necessary, in using the Kirschmann photometer, carefully to fix the position of the observer so that he shall look directly into the long black tube, since the wall reflects some little light. We have found that a large black box (18 by 18 by 36 in.), with a circular hole cut at one end, makes a dark chamber whose sides are not brought into the observer's field of regard by any ordinary accidental shift of position.

III. Urban's Tables and the Method of Constant Stimuli

By E. G. Boring.

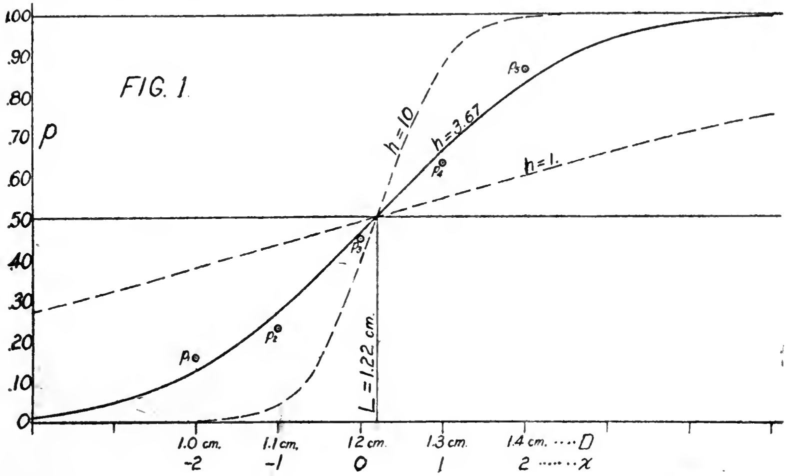
F. M. Urban's recent publications on the psychophysical methods, and in particular his *Hilfstabellen,* have so revised the procedure of the method of constant stimuli that the account in Titchener's *Quantitative Manual* is no longer adequate. In the Cornell Laboratory we have found it necessary to supplement the text of the *Manual* by individual instruction in the use of Urban's tables. The student cannot ordiarily be sent directly to the original articles, for the mathematics (and the German!) are usually beyond him. We propose, therefore, to print an elementary account of the method of constant stimuli in its present form. We shall use little mathematics. The instructor or student will, however, find in the notes at the end of this paper an indication of sources.

§ 1. The Psychometric Function

If for every member of a series of stimuli it is possible to give the one or the other of two judgments, and if it be found that the frequency with which the one judgment is given depends upon the value

[3] Titchener, *op. cit.*, 35ff.

of the stimulus and increases as we pass successively from one stimulus in the series to the next, we may say that the frequency, and hence the probability, of the occurrence of the given judgment is a function of the value of the stimulus. This function is called the *psychometric function.* Thus, if we have a series of separations of the aesthesiometer points, and if we find that the judgment 'two' is given almost not at all at the one end of the series, and almost universally at the other end, and increases in frequency between the two extremes, then a statement of the frequencies of these judgments for the successive stimulus-values is a statement of a psychometric function. The exact form of this function cannot be stated in advance and has never been determined even approximately for most experimental conditions. We know in general, however, that the series of frequencies increases continuously, although not at a constant rate, and that even for extreme stimuli there occur, though very rarely, judgments of the kind not characteristic of the particular extreme. In other words, if we take enough cases in determining the two-point limen, we shall never get quite as low as 0%, no matter how small the separation, nor quite as high as 100%, no matter how great the separation.



There is a mathematical function which satisfies these conditions, and which we might expect in general to apply in such a case; it is a form of the probability curve known as the Φ (γ) [phi function of gamma]. Such a curve is shown as a solid line in Fig. 1, which represents the frequency of the judgment 'two' as given for successive separations of the aesthesiometer (expressed in cm.). Theoretically the curve never quite reaches 0% or 100%, although these values may be obtained in an actual experiment based upon a relatively small number of cases. The presumption in favor of this form of curve lies in the fact that it expresses approximately the frequencies of any measure dependent entirely on chance. If, for instance, we measure the heights of a great many college men, and then plot successively the *per cent.* under 150 cm. in height, the *per cent.* under

151 cm., and so on by cm. up to (say) 190 cm., we get approximately this curve of the Φ (γ). The presumption that this curve would be a psychometric function has been borne out in practice so far as experiment has gone. Accordingly we are justified in making it the basis of our method.

It is clear from Fig. 1 that the theoretical curve or psychometric function is symmetrical about its middle point at 50%. This point is taken as the limen; that is to say, the limen is defined as that value of stimulus for which the probability of the judgment 'two' equals the probability of the judgment 'one.' The fixing of the liminal point, L, does not, however, determine the whole curve. Any number of Φ (γ)-curves can be drawn through this point, every one depending on the particular measure of precision, h, which attaches to it. The greater the value of h, the steeper the curve. For the solid curve of Fig. 1, $h=3.67$. The steeper dotted curve is for $h=10$; the flatter dotted curve is for $h=1$. If $h=0$ the curve would become a horizontal line; the limen would be indeterminate, and its precision zero.

§ 2. Selection of Stimulus-Values

The values of the stimulus must be selected on the basis of preliminary experiments so as to meet several requirements.

(1) The extreme stimuli must not give frequencies too close to 0 or to 100%. We are seeking to determine the point of the psychometric function which corresponds to 50%. Thus the more remote a given frequency is from 50%, the less weight can it be given as an index of the 50%-point. If we actually obtain 0% or 100% (results which violate the Φ (γ)-hypothesis), we are obliged, in computing the limen, to give them a weight of zero; that is to say, the terms disappear and the experimental work with those stimuli is entirely wasted.

(2) The stimuli should turn out to be grouped approximately symmetrically about the limen. Otherwise, the one judgment would be given oftener than the other, and we should run the risk of errors of habituation or expectation.

(3) It might appear from (1) that the best choice of stimuli would be a set in the immediate neighborhood of the limen. We cannot, however, push this argument too far. In the first place, we find that stimuli near the limen most frequently give rise to difficult judgments, and that the usually easy judgments of the more extreme stimuli exert a steadying effect upon the observer. What the extreme values lose in mathematical significance, then, is made up by their effect upon the attitude of the observer. In the second place, we must remember that, even should we wish to take all our stimuli close to the limen, we cannot do so because we cannot prophesy exactly where the limen will be situated. The closer together our stimuli are taken, the more easily will a slight divergence of the results from the anticipated frequencies render the distribution of the stimuli about the limen asymmetrical, and thus violate condition (2).

(4) The stimuli should be separated by equal intervals. The symmetrical distribution of (2) already implies this condition. The use of Urban's tables necessitates it (for the tables consist of calculated values which are based on this assumption).

(5) The number of stimuli chosen should be from five to seven. Experience shows that five is sufficient.

To satisfy the foregoing conditions we must determine by preliminary experiments a stimulus which gives a frequency between 10 and

20%, and another stimulus which gives between 80 and 90%. Then we must choose three other values which occur at equidistant intervals between these two extremes. We cannot afford, however, to employ values which the apparatus in use does not readily furnish. With the aesthesiometer we must use even scale-divisions. It may, therefore, be necessary to take six or seven stimuli instead of five, or to select a value slightly above or below one or both of the predetermined extremes. Even if this change destroys the symmetry of the stimuli about the expected position of the limen, it may be necessary. If the student is in doubt what values to select, he must consult the instructor.

We must never consider the choice of stimuli as final until the entire experiment is completed. After making the tentative choice described above, the values should be tried out in ten series. If these series indicate that the stimuli are likely to give frequencies which satisfy the necessary conditions, we may proceed with the method proper; but we must keep a watchful eye upon the results. If we find as the method progresses that our values were not wisely chosen, we must be willing to consider all our work thus far as preliminary, to choose new values, and to begin afresh. In practice, however, it seldom occurs that the indication of the preliminary series is refuted by the subsequent observations.

§ 3. Experimental Procedure

The directions in the *Student's Manual,* 103f., may be followed exactly. One hundred complete series are required. If Urban's tables are to be used, this number cannot be increased, since the tables are made out for even percentages only, and a greater number of series would give fractional percentages.

§ 4. Problem

The problem of this method may be illustrated by typical data, obtained by an undergraduate pair. In the experiment, five stimuli (D) of 1.0, 1.1, 1.2, 1.3, and 1.4 cm. gave frequencies (p) of 16, 23, 45, 63, and 86% respectively. (See columns 1 and 3 of the Table.) These actual values are plotted as separate points (p_1, p_2, *etc.*) in Fig. 1. Since the observed frequencies fit the Φ (γ)-function approximately, but not exactly, we have to determine the particular Φ (γ)-curve which best fits the results. The solution involves three principles. (a) We avail ourselves of the known properties of our hypothetical psychometric function by making use of a table which gives the relation between the values of p (ordinate) and γ (abscissa). (b) We must apply different weights to the different frequencies according as they are near to, or remote from, the critical 50%-point which determines the limen. (c) We must compute for these weighted values the *most probable* Φ (γ)-curve by the method of least squares.

§ 5. The Φ (γ)-Hypothesis

Let δ represent the (unknown) distance from the limen to any stimulus. Subliminal stimuli will then have negative values of δ; supraliminal stimuli, positive values. The actual values of δ vary inversely with the unit of measurement used. The smaller the unit, the larger is the number which states a given δ. The measure of precision, h, however, varies directly with the unit of measurement, so

TABLE

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
			w	γw	$x w$	$x^2 w$	$x\gamma w$							
D	x	p	P	γP	xP	x^2P	$x\gamma P$	γ	s	sP	xsP	δt	γt	pt
1.0	-2	.16	.6921	-.4866	-1.3842	2.7683	.9732	-.7031	-2.2969	-1.5897	3.1794	-.22	-.807	.127
1.1	-1	.23	.8179	-.4273	-.8179	.8179	.4273	-.5224	-1.4776	-1.2085	1.2085	-.12	-.440	.267
1.2	0	.45	.9943	-.0883	0	0	0	-.0888	-.9112	-.9060	0	-.02	-.073	.459
1.3	1	.63	.9607	.2254	.9607	.9607	.2254	.2346	-.2346	-.2254	-.2254	.08	.294	.661
1.4	2	.86	.6463	.4937	1.2927	2.5853	.9875	.7639	.2361	.1526	.3052	.18	.661	.825
......	......	......	4.1113	-.2831	.0513	7.1322	2.6134	......	......	-3.7770	4.4677	......	......	......

$$h^1 = \frac{(4.1113)\ (2.6134) + (.2831)\ (.0513)}{(4.1113)\ (7.1322) - (.0513)\ (.0513)} = .3670$$

$$L^1 = \frac{(.0513)\ (.3670) + .2831}{(4.1113)\ (.3670)} = .2001$$

$$h = \frac{.3670}{.1} = 3.670$$

$$L = 1.2 + (.1)\ (.2001) = 1.22 \text{ cm.}$$

Check (addition):

$$-3.7770 = .0513 - 4.1113 + .2831$$
$$= -3.7769$$

$$4.4677 = 7.1322 - .0513 - 2.6134$$
$$= 4.4675$$

Check (solution):

$$(7.1322)\ (.3670) - (.0513)\ (.3670)\ (.2001) = 2.6134$$
$$2.6137 = 2.6134$$

that the precision is also always dependent upon the unit. Thus it happens that the product $h\delta$ is independent of the particular system of units used. This product is called γ.

$$\gamma = h\delta$$

γ depends on the frequency, p; and Fechner's Fundamental Table (*Student's Manual,* 99) gives the value of γ for each value of p, thus determining the general properties of the curve, although leaving the form to be finally determined in any particular case by the system of units and the value of h.

It is evident that the curve is completely determined by any two points. For suppose that two separations, D_1 and D_2, give frequencies, p_1 and p_2, and that we find from a table the values of γ_1 and γ_2 corresponding to p_1 and p_2. Then by definition of γ:

$$\gamma_1 = h\delta_1, \text{ and } \gamma_2 = h\delta_2.$$

But, if L is the limen, $\delta_1 = D_1 - L$ and $\delta_2 = D_2 - L$. Thus:

$$\gamma_1 = h\ (D_1 - L)$$
$$\gamma_2 = h\ (D_2 - L)$$

which can be solved simultaneously for the values L and h.

Since in our actual case the five frequencies do not fall exactly on the theoretical line, we should find that every pair would give us slightly different values of L and h. Thus, since our results are slightly inconsistent with our hypothesis, we must presently find the ***most probable*** values of L and h, under our hypothesis, by the ***method of least squares;*** but first we must *weight* our determinations.

§ 6. Weighting

The necessity for some sort of weighting is suggested by inspection of the curve of Fig. 1. The point p_3 (45%) fixes the position of L with much greater definiteness than does the point p_5 (86%). A change of 1% in p_3 would not shift L nearly so far to the one side or the other as an equal change in p_5 would tend to do; for at p_5 the abscissa-change for a unit-change in ordinate is relatively large. The exact values of these weights have been computed by Urban, and can be found from a table. See the column for P in Urban's tables. It will be observed that $P = 1$ for 50% and $P = 0$ for 100%. Such a relation was to be expected. The observed 50% must exert maximal influence upon the determination of the most probable 50%-point. The observation 100% contradicts the hypothesis, and can have no effect at all upon the results.

If we are now to determine the most probable values of L and h, we must write the full set of equations, multiply every equation through by its weight, P, and then apply to the weighted equations the method of least squares. These weighted equations are:

$$\gamma_1 = h(D_1 - L) \text{ with the weight } P_1$$
$$\gamma_2 = h(D_2 - L) \text{ with the weight } P_2$$
$$\gamma_3 = h(D_3 - L) \text{ with the weight } P_3$$
$$\cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot$$

§ 7. The Method of Least Squares

We wish to obtain the $\Phi(\gamma)$-curve which represents our observed percentages with the greatest degree of probability. Such a curve will be, as we learn from the theory of probabilities, the curve for which the sum of the squares of the deviations of observed percentages

from theoretical percentages represented by the curve shall be a minimum. It may be determined by the method of least squares. This method results in the formation of two *normal equations,* which involve the known values of D, P, and γ, and the unknown values of L and h. By solving the equations simultaneously for L and h, the limen is determined. Substituting the more general term, x, for the specific term, D, these normal equations are:

$$[x^2P] \,.\, h - [xP] \,.\, L.h = [x\gamma P] \quad . \quad . \quad . \quad (1)$$
$$-[xP] \,.\, h + [P] \,.\, L.h = -[\gamma P] \quad . \quad . \quad . \quad (2)$$

where the squared bracket indicates the sum of all the values enclosed, computed separately for every one of the stimuli used.

We might now look up in tables the values of P and γ corresponding to every p (*cf.* columns 3, 9, and 2 of our Table), and compute from these figures the values γP, DP, D^2P, and $D\gamma P$ (*cf.* columns 5, 6, 7, and 8) for every one of the five values of D. Since four-place numbers would have to be multiplied together, the work would be laborious and the chance for error great. Fortunately the publication of Urban's tables makes these multiplications unnecessary.

§ 8. Solution of the Problem with the Use of Urban's Tables

Our problem, graphically represented in Fig. 1, is to fit the best curve to a given set of observed points. If we arbitrarily alter the units in which the stimuli are measured, that is to say, the abscissa-scale, we do not change the procedure. L and h come out in terms of the new system of units, and can be changed back again to the old system, or indeed to any other system, as one may desire. In Fig. 1, for example, we may substitute for the values of D, 1.0, 1.1, 1.2, 1.3, and 1.4 cm., an arbitrary system of x-units, *viz.*, −2, −1, 0, 1, and 2 respectively. The only necessary condition is that, since the values of D are equidistant, the values of x must also be equidistant.

In accordance with this principle, Urban has computed in his tables the products, upon which the sums of the normal equations are based, for a set of 15 equidistant x's, *viz.*, −7, −6, −5, −4, −3, −2, −1, 0, 1, 2, 3, 4, 5, 6, 7. But before we proceed to the solution of our problem by the use of Urban's tables, two warnings are necessary.

Arrange your work systematically! Errors are easy to make and hard to find. The Table accompanying this article shows a form in which the principal computations may be kept. But the scratch-sheets on which the additions and multiplications are made should also be kept in order, until the limen is found and checked.

Be careful of your signs! Urban's tables are for positive values of x and γ. The same figures apply when either x or γ or both of them are negative, but the signs of some quantities are altered and of others not. The columns must be summed up algebraically, and attention must be paid to the signs of the sums in substituting in the equations for h and L.

Turning now to our Table, we write the values of D in cm. in column 1. Since the D's are equidistant, we may select for column 2 any set of equidistant x's between −7 and +7. (We *might* use, for instance, the values 3, 4, 5, 6, 7.) We take the values from −2 to +2, because they are small numbers, because they include the simple multiplications by 1 and 0, and because they will give in the long run the fewest negative quantities.

If we write the percentages, p, in column 3, we are ready to fill in columns 4 to 8 from Urban's tables. It is most convenient to fill in a line at a time. Let us neglect, for the moment, the signs. In the first line, $p = .16$. Urban's tables do not read below $p = .5$, because they are symmetrical. The values for .16 are thus the same as those for .84 ($1.00 - .16 = .84$). The values of P and γP for $p = .84$ can be read directly from the second and third columns. Since $x = 2$ (temporarily neglecting the sign), xP becomes $2P$, x^2P becomes 2^2P, and $x\gamma P$ becomes $2\gamma P$. Urban's columns for $2P$, 2^2P, and $2\gamma P$, at $p = .84$, thus give the remaining values in our first line. In the second line $p = .23$, which is the same as $p = .77$. P and γP are found as before. Since $x = 1$, xP and x^2P are the same as P, and $x\gamma P$ is the same as γP. In the third line the last three values become zero, since $x = 0$. In the last two lines, p is greater than .5 and is found directly in the table.

But we must remember our signs! P, the weight, is never negative; a negative weight has no meaning; we cannot weight a thing less than zero. Hence all the values of column 4 are positive. Now we have seen that δ is negative when p is less than .5, for δ is the distance of any point from the limen and must be measured backwards for percentages less than 50%. Since $\gamma = h\delta$, and since P is always positive, γP, like δ, must be negative when p is less than .5. Thus the first three values of column 5 must be put down as negative. In column 6, xP is negative whenever x is negative. The next value, x^2P, is always positive, since neither x^2 nor P can be negative. Finally $x\gamma P$ depends upon both x and γ for its sign. If either x or γ is alone negative, then $x\gamma P$ is negative; but, if x and γ are both positive or both negative, then $x\gamma P$ is positive.

Columns 4 to 8 must be summed up *algebraically*.

If we let h^1 and L^1 stand for the constants of the psychometric function as expressed in the system of x-units (not in cm.), we can find their values from the normal equations, (1) and (2), of the method of least squares. It is simplest to find h^1 first, and then use h^1 in determining L^1. Solving the normal equations simultaneously, we get,

$$h^1 = \frac{[P]\ [x\gamma P] - [\gamma P]\ [xP]}{[P]\ [x^2P] - [xP]\ [xP]}$$

Then, substituting h^1 in equation (2),

$$L^1 = \frac{[xP]\ h^1 - [\gamma P]}{[P]\ h^1}$$

Substituting the sums in these equations (see Table), we find $h^1 = .3670$ and $L^1 = .2001$.

We have now to transform our results into cm. Let d be the number of units of D which correspond to a single unit of x. In this case, $d = 0.1$. Let m be the value of D which corresponds to $x = 0$. In this case, $m = 1.2$ cm. Then,

$$h = \frac{h^1}{d}, \text{ and } L = m + dL^1.$$

Substituting (see Table), $h = 3.760$ and $L = 1.22$ cm. (When $d = 1$, as would have been the case had the D's been expressed in mm., the relation simplifies: $h = h^1$ and $L = m + L^1$.)

§ 9. Checking

We have solved our problem, but we cannot yet be sure that our numerical results are correct. The chance of error is reduced by the use of Urban's tables; but still we may make a mistake in copying, in the determination of a sign, in adding algebraically, in substituting in the equations, or in solving them. If we are working exactly, we must check our results, even at the expense of a great deal of labor.

Checking the Sums. We can check all our work as far as the sums in columns 4 to 8 in the following manner. In column 9 write the values of γ, not forgetting the sign. These values can be obtained from the corrected Fechner table (see note to § 5). In column 10 are values of s, a sum which is arbitrarily defined as

$$s = x - 1 - \gamma \qquad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Attention must be paid to the signs of both x and γ in computing s. In column 11 multiply out the products, sP, and in column 12, xsP. Sum up columns 11 and 12.

Multiplying equation (3) by P, we get,

$$sP = xP - P - \gamma P \qquad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

Summing up these terms,

$$[sP] = [xP] - [P] - [\gamma P] \qquad . \quad . \quad . \quad (5)$$

If we multiply (4) by x and then sum up the terms, we have,

$$[xsP] = [x^2P] - [xP] - [x\gamma P] \qquad . \quad . \quad . \quad (6)$$

Equations (5) and (6) contain in their right-hand members all the sums with which we are concerned. The left-hand members are the sums just found for the purpose of the check. The agreement between the two sides should be correct to three places. In our example we get (see Table)

$$-3.7770 = -3.7769$$

and

$$4.4677 = 4.4675.$$

If (5) fails to check and (6) checks, the error must be in $[sP]$, $[P]$, or $[\gamma P]$. If (6) fails to check and (5) checks, the error must be in $[xsP]$, $[x^2P]$, or $[x\gamma P]$. If both equations fail to check, there is a strong presumption that the error is in $[xP]$, which occurs in both equations.

Checking the Solution of the Equations. We found h^1 by solving the two normal equations simultaneously. We found L^1 by substituting h^1 in normal equation (2). We can check these solutions by substituting the values found for h^1 and L^1 in normal equation (1). In our particular example (see Table) we get

$$2.6137 = 2.6134.$$

Graphic Checking. The plotting on graph-paper of the theoretical curve given by L and h, and its comparison with the position of the points which represent the observed frequencies, constitutes a rough check, which may be substituted for the two foregoing checks if exact results are not required. If the curve as plotted appears to be representative of the observed points, it may be concluded that no gross errors have occurred.

To plot the curve, write the deviations of D from the determined limen (column 13 of Table): $\delta_t = D - L$. From δ_t calculate the corresponding theoretical values of γ: $\gamma_t - h\delta_t$ (column 14). Look up the theoretical percentages, p_t (column 15), in a table of the $\Phi(\gamma)$.

Plot p_t against D to give the theoretical curve. It must cut the 50% abscissa at the ordinate of the limen. Indicate the position of the observed percentages, p, by dots or small circles. The graph will have the form of Fig. 1. It is not necessary, however, to extend it beyond the extreme stimuli.

§ 10. The Method of Constant Stimulus Differences

The application of the foregoing method to the problem of the determination of an upper and a lower *DL* requires little further exposition. The lower *DL* is obtained from the p's for the judgment 'less;' the upper *DL* from the p's for the judgment 'greater.' The 'equal' judgments are not considered separately.

In reporting such experiments it is customary to give the value of the *interval of uncertainty,* which is the difference between the two limens.

$$\textit{Interval of uncertainty} = L_U - L_L.$$

The interval of uncertainty is not necessarily a region of subjective equality; for, if the curve of 'equal'-judgments is skewed, the greatest frequency of the equality-judgment may occur outside of the interval of uncertainty.

There are four possible definitions of the *point of subjective equality.* (*a*) Urban defines it as the point at which *the probability of the judgment 'greater' equals the probability of the judgment 'less,'* i.e., the point at which the two psychometric functions intersect. This point is given by the value $\dfrac{h_U L_U + h_L L_L}{h_U + h_L}$.

(*b*) Frequently $h_U = h_L$ approximately. Then the formula becomes $\dfrac{L_U + L_L}{2}$. On the assumption that this relation holds, the point of subjective equality may be taken as *the average of the two limens.*

(*c*) Considering the psychometric function for the 'equal'-judgments instead of the psychometric functions for 'greater' and 'less,' we may define subjective equality as *the average equality-judgment.* If p_1, p_2, p_3, *etc.*, are the percentages of 'equal'-judgments for the stimuli x_1, x_2, x_3, *etc.*, then the point of subjective equality would be given by $\dfrac{p_1x_1 + p_2x_2 + p_3x_3 + \ldots\ldots}{p_1 + p_2 + p_3 + \ldots\ldots}$.

(*d*) Finally, the point of subjective equality may be taken as *the most probable value of the equality-judgment,* that is to say, the maximal point of the curve of 'equals.' This point is found by taking the three maximal frequencies, p_a, p_b, and p_c, corresponding respectively to the stimuli x_a, x_b, and x_c. If the three values of x are equidistant, the stimulus corresponding to the maximal point is $x_b + \dfrac{(p_a - p_c)(x_c - x_b)}{2(p_a + p_c - 2p_b)}$

In a symmetrical distribution the four measures may coincide. Usually their separate calculation and comparison is of interest.

§ 11. Notes

For the history of the method one should, of course, still read Titchener, *Quantitative Student's Manual,* 275ff. The discussion in the *Quantitative Student's Manual,* 92ff., still applies when Urban's tables cannot be used, provided that certain changes are made in the manner of choosing the stimulus-values (§ 2 of this paper), and that the necessary changes are made in the fundamental table (§ 5 and note) and in the table of weights (§ 6 and note). For Urban's account of the modified method, see *Die Praxis der Konstanzmethode,* Leipzig, 1912, 26pp. This pamphlet includes the 'short-cut' tables in correct form. The *Arch. f. d. ges. Psychol.,* 24, 1912, 236ff., contains the tables (with two mistakes) and a briefer indication of their application. On the method in general, see Urban in *Psychol. Rev.,* 17, 1910, 229ff. This article is based on the fuller accounts in *Arch.,* 15, 1909, 261ff.; 16, 1909, 168ff. S. W. Fernberger's monograph, *Psychol. Rev. Monog.,* No. 61, 1913, gives a clear and readily available account of the use of the method (without the 'short-cut' tables) and also of the method of checking.

Since Urban's notation differs from the older notation adopted by Titchener, we must make a choice at the outset; and, since Urban's symbols are those now current in mathematical texts, we shall select them. In comparing the following discussion with Titchener's *Manual* the student should therefore bear in mind that $\gamma = t$, $p = n$, and $P = w''$ (and, since w' is usually unity, and therefore $w = w'w'' = w''$, $P = w$ as a rule). If we let D still stand for the actual stimulus-values, then we can use Urban's x for the corresponding arbitrary values of the table. L will be the limen.

§ 1. The student must refer again to Titchener's discussion of the law of error, *Student's Manual,* 38ff.

It must be remembered that the use of the $\Phi(\gamma)$-hypothesis is not essential to the method of constant stimuli. On the solution of the problem by the arctan-hypothesis and by indifferent interpolation by Lagrange's formula, see Urban, *Arch.,* 15, 335ff.; 16, 205ff.; *Psychol. Rev.,* 17, 233ff., 257ff.

§ 2. On the adequacy of five stimuli to exact results, even when two limens (an upper and a lower *DL*) are to be computed, see Fernberger, *Amer. Jour. Psychol.,* 25, 1914, 121ff.; *Psychol. Rev.,* 21, 1914, 335ff.

The assumption of our discussion is that a range of frequencies from 15% to 85% is ideal. These values are arbitrary. They are, however, sufficiently removed from 50% to satisfy condition (3); and they are weighted in the computation by ⅔ (.6697), so that they still play an important part in determining the limen, as is required by (1).

The present method differs in several counts from Riecker's procedure in the experiment outlined in the *Student's Manual,* 92ff. (*a*) The inclusion of zero separation (one point) may be required under particular conditions, but there is no more reason for using it than there is for using any of the other values. *Vexirversuche* as controls should not be necessary with an observer trained against the stimulus-error. (*b*) The highest stimulus-value (6 Paris lines) should not have been used, for it gave a frequency of 100%. (*c*) The stimuli should have been equally spaced. (*d*) Too many stimuli were used. Five, or at most seven, would have been enough. (*e*) If we reduce the number of our stimuli, we cannot afford to throw out inversions;

and, besides, every observed frequency has a right to be counted to the extent that its particular weight allows.

§ 3. One does not ordinarily wish to take less than 100 series. The tables will work, of course, for any even factor of 100; *i.e.*, 50, 25, 20 series.

The question arises whether *O* should be told when a new series is begun. It appears at present that, in the most careful work, he should make every single judgment absolutely independently, without any reference to the rest of the experiment; *cf.* S. S. George, *Amer. Jour. Psychol.*, 28, 1917, 1ff. (especially 33ff.). Under George's conditions the announcement of the beginning of a new series would be undesirable.

§ 4. *Cf. Student's Manual*, 102f.

§ 5. Urban has shown that Fechner's table (*Student's Manual*, 99) contains slight errors (from .0001 to .0003) in 15 of its values. The corrected table, calculated from Bruns' table of the probability integral, is printed in *Arch.*, 16, 180; *Psychol. Rev.*, 17, 251; W. Brown, *The Essentials of Mental Measurement*, 1911, 134. The student should correct the table in his *Manual* from one of these sources, and change t to γ and n to p. Although this table is not used in the 'short-cut' method, it is necessary in checking the results (§ 9).

§ 6. Urban's values of the weights, *P*, differ radically from Müller's; *Student's Manual*, 101. In some cases they are twice as great. Urban uses a different formula. For the formula, see *Arch.*, 16, 181; *Psychol. Rev.*, 17, 252; *Konstanzmethode*, 17. The actual values of the weights are tabulated to three decimal places in *Arch.*, 16, 183; *Psychol. Rev.*, 17, 253; Brown, *op. cit.*, 135; and to four places (since they constitute the first column of the *Hilfstabellen*) in *Arch.*, 24, 240; *Konstanzmethode*, 20. Since the student must be provided with a set of the *Hilfstabellen* (see § 8, note), he should simply disregard Müller's table in the *Manual*.

In the equations, P $(=w)$ must affect both sides, if the equations are to hold. The corresponding formulae in the *Student's Manual*, 102, are misprinted; w should apply to the right-hand side as well as to the left.

The precaution in *Student's Manual*, 100, still holds. If different frequencies are based on different numbers of observations, then the equations must also be weighted in proportion to the number of observations taken for every one; but the method of constant stimuli in its usual form prescribes the same number of observations for every stimulus.

§ 8. The tables in the *Konstanzmethode*, 20f., are correct. Those in *Arch.*, 24, 240f., have only two errors in the column for 6^2 P, when $p = .89$ and $.90$. These values are rarely used. See *Arch.*, 25, Literaturber. 84, for corrections. The table is more easily read if ruled horizontally every ten lines. In the Cornell Laboratory we have a negative of the correct table, thus ruled, from which we take blue-prints for the students. The blue-print costs about five cents and can be pasted in the *Manual*. When carefully made, it is quite as legible as the original. We can furnish blue-prints from our negative to any laboratories that may desire them.

The danger of error is minimized by using direct formulae for h^1 and L^1, instead of having the student solve the normal equations simultaneously. Some undergraduates do not remember how to solve simultaneous equations; many make mistakes. Urban gives equations of

similar form, *Arch.*, 16, 186. We have avoided the use of the symbol c ($= hL$), because its exclusion simplifies the discussion and because the mathematical significance of this product (c) is not apparent to the non-mathematical student.

§ 9. On the first check, see Fernberger, *Psychol. Rev. Monog.*, No. 61, 32ff.; Urban, *Konstanzmethode*, 24f. Fernberger's statement that the check should be exact applies to the long method. The discrepancy in the fourth decimal place in our example arises from the fact that the values in Urban's tables are given to four places, but are computed from five-place P's and γ's; whereas our values of sP and xsP are based on four-place P's and γ's.

The second check unfortunately involves a multiplication ($h\,[xP]$) which was made in finding L^1. The second term of the left-hand member is therefore best found by multiplying in the order $L^1 \times [xP] \times h^1$.

While the graphic check is rough, it has the great pedagogical advantage of showing the student diagrammatically just what the method has accomplished. It is very simply applied and is especially useful when the student's time is limited. We ordinarily use this check in the Cornell drill-course, whether or not the others are omitted.

Fechner's table may be used to change γ_t into p_t. Since the graphic check is rough, the table in the *Students Manual*, 99, can be used even if uncorrected. It is much more convenient, however, to employ a table that reads from γ to p, instead of from p to γ. Such a table is to be found in B. Kämpfe, *Philos. Stud.*, 9, 1893, 147ff. (not *Psychol. Stud.*, as Urban says); and in H. Bruns. *Wahrscheinlichkeitsrechnung und Kollecktivmasslehre*, 1906. It is from the Bruns table that Urban computed the corrections of the Fechner table. Both these tables give values of Φ (γ), which must be changed into p by dividing by 2, and adding to .5 when γ is positive, or subtracting from .5 when γ is negative. We have the Bruns table on two negatives in the Cornell Laboratory, and can furnish blue-prints of them. For the rough graphic check the Bruns table is unnecessarily accurate. Miss J. M. Gleason has prepared a mimeographed table which gives p directly for values of γ to two decimal places. Miss Gleason has deposited the stencil and a large number of copies of this table with the Cornell Laboratory, so that we can also furnish limited numbers of this table to those who may desire them.

It may be pointed out that, if the values of γ_t are computed accurately and the values of p_t found accurately by interpolation in the Bruns table, then the sum of the squares of the differences between p_t and the observed percentages, p, constitutes a measure of the degree with which the actual case fits the hypothesis.

§ 10. The formula of (a) is easily derived if we take x_e as the point of subjective equality, p_e as the percentage of the point of intersection (subjective equality), and γ_e from the table for p_e. Since γ_e is common to both psychometric functions, it can be expressed with respect to both; thus,

$$\gamma_e = \delta_e{}_L h_L = (x_e - L_L)\ h_L,$$
$$\gamma_e = -\ \delta_e{}_U h_U = (L_U - x_e)\ h_U.$$

Equate the two right-hand members and solve for x_e to get the formula. See Urban, *Arch.*, 16, 201.

The formula of (*d*) is found by assuming that a parabola may be used as the interpolated curve (a parabola is determined by three points), and finding the maximum by equating the first derivative to zero. For the derivation, see Urban, *The Application of Statistical Methods to the Problems of Psychophysics,* 1908, 124f. The formula given here has A—C in the numerator and is correct. It has been misprinted A + C in *Arch.*, 16, 187, and *Psychol. Rev.,* 17, 236ff.; but with this caution in mind, see those discussions.

For examples of asymmetrical psychometric functions, see *Arch.,* 16, 199ff.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

Communicated by E. B. TITCHENER and H. P. WELD

XXXIV. SIZE *vs.* INTENSITY AS A DETERMINANT OF ATTENTION

By J. N. CURTIS and W. S. FOSTER

This study is an attempt to compare the attention-compelling power of size and intensity in the case of Greek crosses. A standard cross, identical with that used by Meads [1], the arear of which was 56 sq. cm., was compared with two similar crosses whose areas were respectively 28 and 112 sq. cm.

The apparatus used was that of Meads, save that a pendulum tachistoscope was substituted for the spring tachistoscope. The average time of exposure was also the same (110 sigma), but the mean variation of this average was reduced from 8 to 3 sigma. A 40-watt Mazda lamp was used, and the standard cross had the intensity of 225° of light.

Preliminary experiments by the method of limits indicated that results as definite and constant as those in which 'form' and intensity were compared could not be obtained for size. To rule out, so far as possible, any influence of expectation, we turned, in the regular series, to the method of constant stimulus-differences. The experiments were arranged to compensate for the irregular influences of practice and fatigue, and to measure the error of space.

The observers were Dr. E. G. Boring, Dr. W. S. Foster, and Mr. F. L. Dimmick, all highly practised. All observers completed 200 series; 50 with each of the two comparison crosses in each of the two spatial positions, right and left of the standard. The period of observation was approximately an hour in length, and in general gave time for ten series. Rest-periods of three to five minutes were allowed twice during the hour. The observer was not told the number of steps in a series, nor did he get any indication of the point at which one series ended and another began.

The instructions, which were read at the beginning of every experimental hour, were: "At 'now' put your attention definitely upon the fixation-point. Two crosses of unequal size will be exposed. Judge which of them, if either, is the more *clear,* i. e., which one of them

[1] *Am. Jour. Psych.* xxvi., 1915, 150.

catches your attention the more." The observers judged: "Right," "Left," "Equal," or "Doubtful," but doubtful judgments were rarely given, and are counted as equal. Introspective reports were not required, but were occasionally volunteered.

At the beginning of every hour a 'warming-up' series with steps of approximately 30° were taken, in which the values of the episcotister-opening for the comparison-cross were varied at haphazard over a very wide range. Such a preliminary series seemed essential, in order that the observer should receive no suggestion from the experimenter as to what region was expected to be critical. The observer knew that this series was taken for purposes of orientation, as well as for 'warming-up.' The judgments of the preliminary experiments exhibited regularity, and served as a basis for determining the range and position of the succeeding regular series. The latter could now be made out in seven to ten steps of 15° with fair assurance that their range and position were appropriately chosen. The percentages were computed separately for each group of ten series. In only 17% of the computations was the low percentage for the one end-stimulus as great as .20, and in only 11% of them is the high percentage for the other end-stimulus as low as .60. Even in these few cases, such percentages are usually inversions. We have assurance, therefore, that the range of values covered on a given experimental day is in general sufficientlv great.

The position of the stimulus-series thus indicated for succeeding days, however, turned out to be much more variable than we had anticipated. In the accompanying chart the intervals of uncertainty for the various observers and for the two spatial positions are represented by heavy vertical lines. Their order from left to right is the order of practice, and their positions are evident from the ordinate-numerals which are expressed in degrees of light of the comparison-cross. Broken curves connecting the succeeding points of subjective equality are for the comparison-cross on the right; solid curves are for the comparison-cross on the left. After nearly half of the experiments had been completed, it proved that even full intensity of the comparison-cross was sometimes insufficient to make it as attractive for attention as the standard. In such cases, without knowledge on the part of the observers, the experimenter decreased the intensity of the standard, which accordingly became the variable. In the chart the values for the interval of uncertainty thus obtained are represented as if a *proportionately* increased intensity had been given to the comparison-cross, though of course such representation is not strictly justifiable.

The dots enclosed in circles represent the averages of the points of subjective equality for each observer, the two spatial positions being combined. The effect is that of combining the broken and solid curves for a given observer and a given size of cross and reducing them to a single point. The figures following the ± sign are the mean variations of these points from the averages.

If we may trust these gross averages, it seems that the larger cross must be slightly more intense than the smaller in order to exert equal power over B's attention. Moreover, the smaller cross which is only half the area of the standard catches his attention equally well if its intensity be increased by only 8° of light; indeed, if it is placed to the left of the standard, it catches his attention at a much lower intensity.

For F, size seems in general to have very little attention-compelling power. The smaller cross must be increased by 67° to become equally

clear, but the larger cross catches his attention at an intensity no whit lower than the standard, which is only half its size. For both B and F, therefore, the standard appears to have been of a size to compel attention somewhat more easily than either a smaller or a larger size.

D requires that the smaller cross shall be much more intense, and the larger considerably less intense than the standard, if they are to catch his attention equally well.

In the case of one observer (B), spatial position makes a greater difference in attention-compelling power than a four-fold increase in area. In terms of the chart, that is to say, his two broken curves occupy more nearly the same position than do his broken and solid curves for a cross of a given area. His M. V.'s accordingly are large (110 and 102°). In the case of the smaller cross another observer (D) also has a large M. V., though in this case it is dependent not on position but upon wide daily variability, which in a lesser degree is characteristic of all observers. All of these facts, taken together, added to the facts of wide individual differences, seem to us sufficient indication that, under our conditions, *size is not a definite determinant of attention.*

We have no explanation for the apparent relative stability of the interval of uncertainty during the single experimental hour and the wide variability of its position upon different days. Since its position was roughly determined each day by preliminary experiments over a wide range of possible positions, and since within the groups of ten series there is a fair degree of regularity in the percentages, we do not think it possible that the experimenter's choice of values, and a self-imposed tendency to give approximately equal numbers of "Right" and "Left" judgments, were responsible.

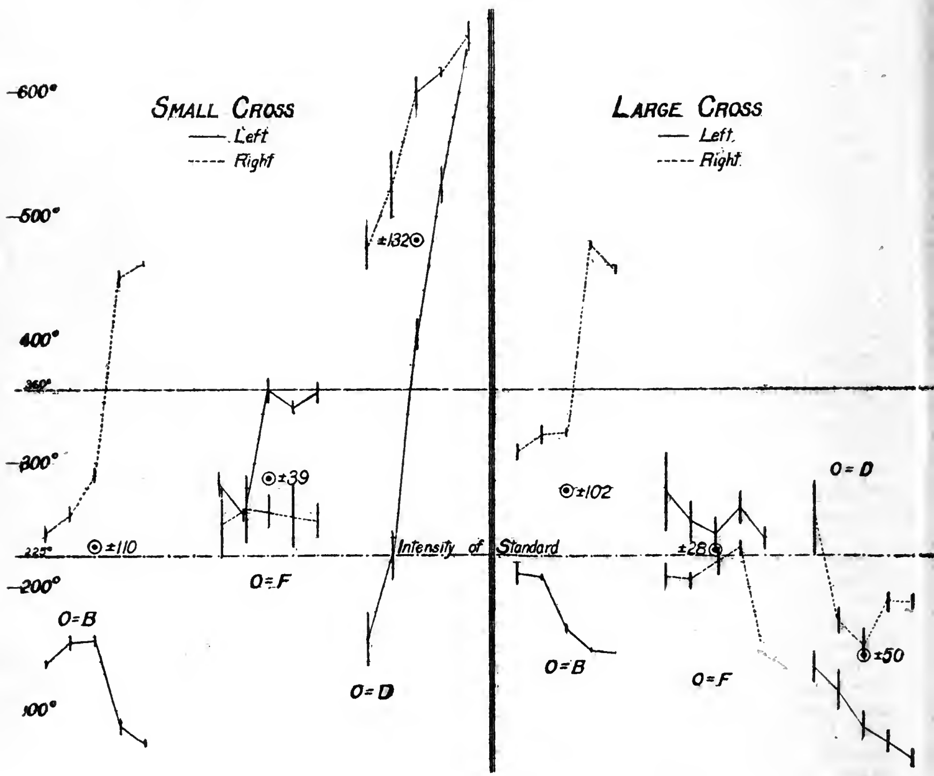
Our observers, find, as a rule, that the judgment is made without difficulty. At first B was not certain that his judgments were judgments of clearness alone, and thought that the natural judgment was perhaps one of clearness and intensity combined. At first he tried actively to make the judgment solely in terms of clearness; later he takes the experiment more passively, and finds that as he does so "an intensity-clearness fusion seems to be built up. Certain intensity values force themselves in, under passive attention, as invariable concomitants of the clearness." At first he was also somewhat disturbed by the fact that at times the crosses (especially the larger one) seemed unevenly illuminated over their surfaces. After a few hours, however, he no longer mentions such difficulties, and settles down to what is apparently a constant attitude. He does not seem to realize that the smaller cross at even lesser intensities is often judged clearer than the standard. He does remark that, when the smaller cross is the clearer, it is a "bright, cute, mean little thing" and gives him the "cute little devil" attitude. He suspects that "what really happens is that, when the little one does catch attention, I'm surprised that it should in spite of being so little. I know that I'm not disposed for the little one; quite sure I do not anticipate it. The smaller cross *snaps* at me; the larger one rather *swoops* at me."

F finds no difficulty in making an immediate judgment, except when the crosses seem to be equally or nearly equally clear, and he volunteers few remarks.

D was somewhat disturbed at first by the fact that all three crosses seemed to change in size from experiment to experiment within a series, although he knew that objectively they were constant. The clearer cross often seemed to be closer to him in space, and also to come earlier in time.

Our results were obtained under special conditions and with few observers. So far as they go, however, they show that size does not possess the definite compelling power of form and intensity, and that other factors must have been at work to produce the individual differences. If this result is confirmed, the comparatively uniform influence credited to size in the case of advertisements[2] must be regarded as doubtful; other factors than the apparent factor of size may there have been in play.

[2] See *e.g.*, H. F. Adams: *Advertising and its Mental Laws,* 1916. Cf. W. D. Scott; *Psychology of Advertising,* 1912, 157 ff; H. L. Hollingworth; *Advertising and Selling,* 1913, 67; D. Starch, *Advertising,* 1914, 61.



A REMINISCENCE[1]

By G. Stanley Hall

Brethren: Our president's invitation to me to speak at this anniversary banquet calls for reminiscence. Dr. Wiley of pure-food fame, who ought to know, says that the older a man is, the better he is or should be unless senile involution has reached his psyche. Having for some months not very successfully or pleasantly wrestled with the problem of trying to realize what three-score and ten means, besides the ideal view that it is the youth of old age I feel that I can already report some progress toward realizing not only that early senescence is not so bad as it is painted, but that its study is likely to prove even more interesting than that of adolescence ever was. I am, at any rate, rather glad to overcome my horror of anecdotage and of being personal, in the effort to make the younger members of this association realize, in some respects at least the difference between the rather painful past of psychology in this country and its splendid present. My memories in the department our now broadened association represents go back to college days under Mark Hopkins, who taught us a mixed system of theology and metaphysics so simple and final that very few indeed of his many generations of pupils ever went any farther than he left them. In his later years he confided to me that he had once tried to read Bohn's translation of "Mr." Kant, but never got beyond the first paragraph, which he could not understand. His scheme of the world provided ready-made answers to all great questions, and pretty effectually inoculated his pupils against any more serious infections by the muse of philosophy, who was always represented as dangerous because trust in reason was so liable to mislead the soul.

We had a touch, but not too much, of Sir William Hamilton, Jouffroy, a few dangerous *aperçus* into Hickok, and because I composed a crude essay on John Stuart Mill and developed an ephebic calenture for Carlyle and Emerson, the latter of whom was given a very cold reception when he gave a course of lectures in town, I was regarded upon graduation as distinctly out of the fold.

The next year, in New York, I most happily fell under the influence of George S. Morris, the translator of Überweg, read his proof sheets, and found that the history of philosophy, which I had been taught was a pitfall of skepticism leading men to hold any, every, or no opinion, made such an appeal to me that I went to Europe for three years. I studied with Trendelenburg of Berlin, the great Aristotelian of his day, with Zeller and Kuno Fischer at Heidelberg, and with Benno Erdmann, an eclectic, at Bonn. I followed Dorner, who represented Schleiermacher, and Karl Rosenkranz, the last of the Hegelians, getting a touch of Herbart, who was then represented by no less than three professors at Leipzig, and seeing incidentally the War of 1870

[1] Read at the 25th meeting of the American Psychological Association, New York, December, 1916.

from the German side. On returning home, feeling myself very advanced, I offered my services to various colleges and universities but in vain, till a state institution in the middle west gave me, as I thought definitely, an appointment. Three months later, however, I had a letter from the president stating that it had been decided, after maturer deliberation, that a man who had studied the history of philosophy in Germany would probably be more or less infected with pantheistic tendencies, and that to teach the systems of different men, rather than one definite system, would unsettle young men, and that therefore the engagement must be canceled.

Then came a year of anxious and watchful waiting, and finally, as a professor of modern languages in a small but liberal Ohio institution, I was kindly permitted by the Unitarian president to teach philosophy on the side as an extra, in his place. Then came a period of study and teaching at Harvard, under Bowen, Everett, Hedge, Elliot Cabot, Palmer, and James, the latter of whom had a tiny laboratory under a stairway in the Agassiz Museum, containing a metronome, several optical charts, diagrams of the brain, ear and eye, with a dish for rotating a frog. Meanwhile I was doing more or less serious work on muscles in the physiological laboratory of the Harvard Medical School under Henry P. Bowditch, pioneer of the new physiology in this country. Meanwhile, too, the first one-volume edition of Wundt's psychology had appeared, which together with Fechner's work, Helmholtz's on the eye and ear, and a few other things, I studied under the guidance of and for several years in the closest intimacy with the charming and inspiring personality of James.

Then followed a second triennium in Europe, with a little work in Wundt's laboratory, just opened at Leipzig, but doing also a slight piece of work with Helmholtz, and much more in Ludwig's laboratory, where I was associated with Von Frey, Gaule, Flechsig, Von Kries, and others. Wundt and his new work were then looked on not only with suspicion but with active criticism by his colleagues. He had been dismissed as an assistant by Helmholtz because of his lack of mathematical training and of severe scientific method, as Helmholtz thought, while physiologists and medical men generally regarded him as an interloper in their field. Reaction to and compensation for this long period of harsh criticism, aggravated by the fact that Wundt had been elected to his position in Leipzig after the severest competition with Horwicz, doubtless had much to do with the, as I think, hyper-development or affectation of a methodology in our domain modeled too much on the ideals of physics, a field where biological affinities are closer and much more suggestive.

Before returning to this country, although I had an academic appointment awaiting me here, fearing a repetition of the former disappointment, I spent two or three months in hasty visitations to many educational institutions, feeling that in this domain, at least, I might make certan applications of philosophy which would supply me a livelihood. This was fortunate, because I had hardly landed when another letter from another college president informed me that as he had learned I had been studying psychology in a laboratory, I could not help being too materialistic to be safe. Hence the very slight and superficial knowledge I had acquired of education came in good stead during a critical and anxious year.

Johns Hopkins, then perhaps almost at the acme of its leadership, decided with great hesitation, as I was afterwards told, to give ex-

perimental psychology a try-out, very tentatively however, with an appointment first for six months, then for one year, then three, then five years, with an appropriation of one thousand dollars a year for apparatus. Dr. McCosh from Princeton severely arraigned the institution for taking this step, and attacked scathingly various things that I had said. My predecessors there, older and far more competent than I, Charles Pierce, George Morris, and even James, who had given several courses of lectures there, had attracted a few students of great promise,—Jastrow, Cattell, Dewey, Patrick, Noyes, Taber, Sanford, Burnham, Motora, and others; and I was even enabled to have Donaldson as an assistant in the domain of neurology, of which I knew little.

Here, too, I had to occupy the unique position of Superintendent of the Bay View Insane Asylum, as *locum tenens* before the opening of the medical school. This I had to visit and inspect twice weekly, taking the responsibility of receiving and discharging patients, holding clinics, etc. In this environment, with the stimulus of trying to make good to colleagues, president and trustees a new department which I knew was an experiment, and against which the same prejudices existed as were felt toward Wundt at Leipzig; stimulated, too, by a perhaps almost unprecedented group of able students; in the exhilarating atmosphere of a university itself new and in some sense itself a great experiment; one found the very highest possible incentive for the hardest kind of work.

One Sunday afternoon I received a call from a wealthy stranger from Philadelphia, J. Pearsall Smith, who had heard of a new department of psychology, and who suggested that I start a journal, handing me on the spot a check for five hundred dollars to that end, with the intimation that more would follow. It proved that his interest centered in psychic research, which the Journal criticized, so that his contribution was never repeated. This was the origin of the American Journal of Psychology, which I began with such great expectations and printing so many thousand copies that the first number, when it was printed, circularized and distributed, cost over fifteen hundred dollars. Soon I found two bitter disappointments. The beginning of the second year showed only a little over one hundred subscribers, and an unexpected dearth of material, so that anyone who turns to the early volumes will find a very large part of them made up of innumerable book reviews, other material written by myself, a large part of the large-type material from my own laboratory, and still other articles in allied fields or of rather inferior quality. There was nothing to do, however, but to persevere, although at the end of a few years I had sunk over eight thousand dollars of my own hard earnings in the Journal, which only five years ago began to show a clear balance, the first year of thirty-one dollars to the good. In those days there was no usable text-book in psychology in English, so that when Ladd's book appeared in 1886 it was a godsend.

This Association was started twenty-five years ago in response to an invitation from Clark University, that had then just been organized. The first members of the Association at this meeting were Angell, Baldwin, Bryan, Burnham, Cattell, Cowles, Delabarre, Dewey, Fullerton, Griffin, Hall, Hume, Hyslop, James, Jastrow, Krohn, Ladd, Nichols, Noyes, Patrick, Royce, Sanford, Scripture, Witmer and Wolfe. The new members elected at this meeting were Wesley Mills, Münsterberg, Ormond, Pace and Titchener.

At the second meeting, at Philadelphia, it was my pleasant function

as presiding officer to introduce to the Association for the first time Professor Münsterberg, who had established himself during the year at Harvard. The early history of the Association has been admirably written by Professor Buchner, and here my bald narrative may well stop.

Cattell, who had done such brilliant work in Germany and England, soon started a laboratory at Pennsylvania under very favorable auspices; Jastrow at Wisconsin; Baldwin was already doing signal work at Toronto; Dewey at Michigan; Titchener at Cornell; of course Ladd at Yale; and now there are hundreds of laboratories, academic and non-academic, where studies more or less psychological are made. American psychologists are falling into natural groups according to their tastes and abilities, representing introspection, behaviorism, study of animals, geneticism, border-line, pathological and anthropological work (in which I think we ought to include the new realism, with its pragmatic trend); and there are waves of new interest represented by tests, standards, and the great corporation movement, which realizes that the greatest natural resource of a country is men fitted by native attributes to their callings. Thus the prospect of psychology to-day in this country is unprecedentedly bright, and we seem well able to accept the enormous new responsibility for leadership that is now laid upon us by the shortage of scientific output owing to the war in Europe. Henceforth this country must take a new leadership in this field, and those interested in various special lines should realize that psychology has become vastly too large to be represented by any individual, group or school, and that specialization, already so well developed that none of us is able to understand all the papers read in our meetings, will go on, for in a sense that none of us ever dreamed this seems destined to become a psychological age, and there is a sense, too, in which democracy can never become complete without intensive individual psychology. While we deplore the loss of the great Harvard trio, James, Royce, and Münsterberg, younger men are developing who will without doubt make good their places, and more. A new science like ours ought in a general way to attract ever better men, so that the younger you are the better you ought to be. Measured in this way, American psychology to-day has everything to hope and nothing to fear.

THE JAMES-LANGE THEORY IN LESSING

As supplementing Professor Titchener's "An Historical Note on the James-Lange Theory of Emotion,"[1] the following passage from Lessing's *Hamburgische Dramaturgie* (*Drittes Stück. Den 8 Mai 1767*) may be of interest. Lessing in the passage referred to distinguishes between actors who genuinely feel the emotions they are called upon to portray without seeming to possess them, and actors who appear to have them without really feeling them. Lessing prefers the latter. "Feeling," as he says, "is something inner of which we can judge solely by its outer signs." A good actor is one who, despite his inner indifference and frigidity, has a mastery over the expressions of passion. If he but learns to observe and to imitate successfully its outer marks he will soon rise to an appreciation of its inner meaning. I translate Lessing's words: "After imitating long enough the acts of others, he (the actor) soon acquires a number of small rules according to which he begins to act independently and by whose observance he gets an emotion (in virtue of the law that those modifications of the soul which bring about certain bodily changes are in their turn affected by these bodily changes) which to be sure cannot have the duration and the ardor of an emotion initiated in the soul but which at the moment of experience is strong enough to produce some of those involuntary bodily changes whose presence alone perhaps vouchsafes the inference of the inner feeling. Let such an actor, for instance, be called upon to portray an extreme fit of anger. I shall assume that he lacks sufficient understanding of his rôle. I shall assume that his own soul cannot be moved to anger because he is unable either fully to comprehend or vividly to imagine the reasons for this passion. And I say: if he has learned to copy the crudest expressions of anger from an actor capable of this emotion—the precipitate walk, the stamping foot, the hoarse voice with its shrieking or sullen sound, the play of eye-brows, the trembling lip, the gnashing of teeth,—if, I say, he but well imitates these things which anyone who desires can imitate, then an obscure emotion of anger will unfailingly come into his soul reacting in its turn upon the body and producing there those changes which are not dependent upon our will, i. e., his face will glow, his eyes will flash, his muscles will swell; in short, he will seem as one truly in anger without being angry or without in the least comprehending why he should be angry."

The University of California. J. Loewenberg.

[1] This Journal, Vol. XXV, 1914, pp. 427-447.

BOOK REVIEWS

The Fundamentals of Psychology. By W. B. Pillsbury. New York, Macmillan, 1916, vii+562 pp.

The fundamentals of psychology, according to the preface, are "the results of experiment and the generally accepted body of facts" of the science. These facts can be stated without dogmatic reference to theory; indeed, theory is to be avoided save in so far as it illuminates fact or emphasizes problems. The introduction deals very briefly, therefore, with method and subject-matter. The method is two-fold. Observation, its objective and experimental side, gives the phenomena as they present themselves to the onlooker, and makes measurement possible. Introspection, or self-observation, gives the phenomena as they appear to the individual investigated. This side of method is apparently secondary: "it will in many cases supplement the results of direct observation;" "it is nearly always suggestive;" and it "may at times furnish a solution to the questions raised by objective results."

For subject-matter the author prefers the term behavior, because "consciousness tends to imply something removed from observation, something mystical," because "behavior is the more inclusive term," and because of "the doubt expressed by recent writers as to whether consciousness exists, at least exists for them individually." No one of these reasons seems logical, even on the writer's own showing. Why should consciousness tend to imply something removed from observation, when we are assured that "the second and third definitions [consciousness, behavior], are alike in that neither implies any theories concerning what is not open to observation?" We are not told how to understand the *inclusive* term behavior. It is defined as "the activity of man or animal as it can be observed from the outside, either with or without attempting to determine the mental states by inference from these acts." Another paragraph limits psychological study to "intelligent" behavior. The body of the book, however, deals specifically and continuously with consciousness and the "materials" of consciousness, and does not deal, explicitly at least, with behavior itself. Apart from the introduction, the author gives no indication that he wishes to deny the existence of consciousness. Except for its discussion of the kinds and fields of psychology, the introduction seems therefore as likely to confuse as to enlighten the student. On the basis of the statements in the preface, indeed, no introduction should have been necessary.

Two chapters deal with the nervous system in greater detail than is usual in text-books at the beginner's level. The treatment is clear and logically ordered; and the illustrations and schemata have the special merit of showing, for the most part, only such details as are made significant in the accompanying text. It is, however, not easy to see just what psychological facts are better understood from a knowledge of the gross and microscopical structure and development of the neurones, or from an ability to trace tracts in the cord and brain and to name and localize lobes and fissures and sensory areas;

and it is curious to note how infrequently the writer refers back in later chapters to these discussions, whether by page or less directly.

The remaining chapters treat of sensation (two), images and the laws of centrally excited sensations, attention and selection, perception (two), memory, reasoning, instinct, feeling and affection, emotion and temperament, action and will, and the self.

The writer's intention to draw freely upon the work of all schools finds expression throughout the book. It leads him in many cases to state rival opinions, rather than to attempt selection and reconciliation. In vision a 'four-color' theory is found necessary, but the Hering, Helmholtz, and Ladd-Franklin theories are all detailed and partially criticized. In addition the views of Köhler, Révész, Meyer, Shambaugh, Helmholtz and others are given, and reconciliation is found difficult or impossible. The doctrines of perseveration and of actual synaptic connection are accepted in explaining memory and association. On the structural side, attention is treated as a complex state involving many different changes; on the functional side, it appears to be the equivalent of selection or of "importance for consciousness." It is explained as a preparation of special neural tracts. A very general empirical (genetic) theory of space perception is accepted—space is said to offer no problems for the nativist—but although special theories are outlined, no serious attempt is made to estimate their adequacy as explaining the total body of facts. Both after-images and retinal streaming are discussed as explanations of the perception of movement, and the former is held to be at present the more probable. Motor and attentional theories, taken together, explain rhythm; and rhythm, strain and memory explain the perception of time at large, according to the duration of the objective interval. The theories of immediate quality and of association are discussed as the explanation of recognition; the author leans toward the latter. A special point is made of recognition as a stage in the development of meaning. Meaning itself may be imageless, and yet conscious. When imageless, it is to be explained as due to a subliminal arousal of appropriate associative tendencies. Instincts are nervous dispositions like the selective tendencies in attention, but generally of wider scope. Affection as sensation and as attribute is criticized; plural theories are discussed and rejected; pleasantness and unpleasantness as the sole qualities are accepted. Lipps' and Stout's theories are mentioned, and emphasis is placed upon the apperceptive doctrine of Wundt and its physiological correlate, though in the end it is maintained that the facts cannot be brought to harmonize with any general statement. The views of Darwin, of McDougall and of James and Lange, and the experiments of Sherrington and Cannon upon emotion are discussed, but only a very general summary is attempted. Thorndike's and Watson's theories of learning are outlined; and Barret's and Michotte's theories of will are compared, to the advantage of the former.

On the side of fact a number of inaccuracies may be mentioned. For example, the color-pyramid is not taken entirely as a psychological, but also as a physical and a physiological construction. It is said to represent the spectral colors and the purples, and the writer neglects the fact that after-image colors of better saturation than those of the spectrum are possible. Perhaps for this reason the *green* and yellow corners are said to be represented as higher than the other two, though the figure raises only the yellow and depresses the blue. The square base does not either represent the facts of complementarism. The

(physiologically) primary colors are not, as stated, red and green, but a red outside the spectrum and a decidedly bluish green. In audition 30,000 to 50,000 is given at the highest rate at which vibrations can be heard. Thus both the fact that beyond these rates noise is still heard, and the fact that musical quality (according to Köhler) ends at approximately 20,000, are neglected. Köhler is also inaccurately stated to identify musical quality with vocality. Combinational tones are said to be entirely of subjective origin, and beats are said to be carried by an intermediate tone, which is only sometimes the case. Rubin's finding of paradoxical warmth is not mentioned. We are not told why the difference-limens are to be regarded, not as constant values obscured by varying conditions, but as fundamental differences due to differences between individuals, nor how this view is related to present theory of psychometric functions. Eight, and not six, single metronome beats without rhythm are said to limit the range of attention. Ebbinghaus is quoted as having found every repetition to produce the same effect in memory; but Ebbinghaus' own qualifications in the case of large numbers of repetitions, and the qualifications of others with regard to the first few repetitions (Meumann's first stage of learning), are not added. Without apparent justification Jost's law is interpreted to mean that associative tendencies actually grow *stronger* (not merely 'set') for some time after learning, and the author represents the curve of forgetting as a resultant of a *rising* curve of association and a rapidly falling curve of perseveration. Ebbinghaus' explanation of Jost's law is not mentioned, and Ebbinghaus' discussion of "slowly learn, slowly forget" is by no means adequately presented. It is stated that nonsense-material is no more easily recognized than recalled, and that recognition always comes when associates are aroused. Heine's demonstration that different factors are involved in recognition and in recall is not mentioned.

The author's generalizations are not always in accord; we have had instances of this in the introduction. For instance: "All the materials of our consciousness are derived from sensation," and "It [affection] is not a mere attribute or phase of the sensation as are quality and intensity; rather we must regard it as a separate mental state or process with attributes of its own": "One can obtain grey, . . . only by the mixture of complementary colors," and "All lights of low intensity appear colorless," "The [totally color-blind] sufferer sees no colors, but only greys," "The sensation [under adaptation] begins to diminish in intensity . . . till all colors disappear": "There is a large number of odors, . . . each of which probably has its own sense organ" and "A case might be made out for separate taste buds for each quality; there is slight evidence for separate organs for each odor": "While each of these processes [feeling, pain, movement], together with taste, mixes with odors to produce a percept that is not immediately analyzable, they do not constitute true odors," and "The only perception that approximates the bare combination of sensations into a larger or more complicated whole is the tonal fusion."

These inaccuracies, of course, appear as minor matters when compared with the general success of the author in assembling the varied results of experimental investigation. The lack of 'system' and catholicity of treatment have their own advantages. Open-mindedness and breadth of view may be favored, but so may also confusion and failure to establish for the student a background from which to criticize the new and the common-sensible. Certainly the author has not been led by his principles to a marked avoidance of theories.

W. S. FOSTER.

The Belief in God and Immortality. By JAMES H. LEUBA. Boston, Sherman, French and Company, 1916, 340 p.

The author defines this to be "a psychological, anthropological and statistical study." It is psychological in that it questions the human intellect and emotions; it is anthropological in that it probes into the savage as well as the civilized mind; and it is statistical in that it makes a numerical examination of the intellectual classes in the United States to-day. Its purpose is to determine how far the belief in God and immortality as it is held at the present time is essential to religious and moral progress. The anthropological part of the study contrasts the belief of savages, which it calls "primary," with the belief of civilized men to-day which it calls "modern," and derives from that contrast the conclusion that the savage belief is a product of fear while the civilized belief is the result of desire; that the savage fear arises from dreams and visions while the civilized desire has for its cause the effort to realize ideals and satisfy affection; that the savage mind, fearing ghosts, has no wish to become one; but that the civilized mind has so ardently desired to "continue" after death as to try, one after the other, a number of methods to make valid this desire in an established faith, but only to abandon them as the boundaries of knowledge have enlarged.

The metaphysical method has been found wanting because it proceeds deductively. Its weakness lies in the untenable character of the general assumptions with which it has to begin. The scientific method, which proceeds inductively, is inapplicable because of the lack of trustworthy data, whether physical or psychical. And the method of "direct 'inner experience'" is not valid for the reason that it rests upon the assumption that a sense of personal well-being signifies personal immortality. A feeling of personal well-being is not even an invariable sign of personal health.

Perhaps the most startling part of this work is the latter half. This is the part which is "statistical" in its treatment. It brings to book what it considers the representative classes of this country by examining "college students, physical scientists, biologists, historians, sociologists and economists, and psychologists." The examination was conducted by means of a *questionnaire* so framed and addressed as to relieve it of the suspicion of unreliability which admittedly attaches to most *questionnaires*. The point to be determined was, what proportion of these groups believe in God and immortality—the term God meaning a personal God, or one "to whom one may pray with the expectation of receiving an answer," and the term "immortality" meaning "personal" or "conditional immortality." The net results are: College men, 56%; scientists, 45.2%-55.5%; historians, 48.3%-51.5%; sociologists, 46.3%-55.3%; psychologists, 25.2%-19.8%.

The answer to the fundamental inquiry of the investigation is that a belief in God and immortality as it is held at the present time is not only not essential to religious and moral progress, but it is disadvantageous. It is, indeed, a positive hindrance, because the doubt surrounding it "creates in the upper intellectual circles of the Churches and more particularly among professors and students of theology a situation threatening the most precious possession of teachers and students: their intellectual integrity." A knowledge of the ultimate is not necessary to moral progress. Society may be trusted to generate the impulses and ideals demanded for each stage of advancement. The urge forward and upward will be greater and not less. It is enough to see as far as the horizon.

Clark University. H. C. GRUMBINE.

RECENT FREUDIAN LITERATURE IN ENGLISH

Mechanisms of character formation; an introduction to psychoanalysis. By WILLIAM A. WHITE. New York, Macmillan Co., 1916.

The history and practice of psychanalysis. By PAUL BJERRE. Authorized translation by Elizabeth N. Barrow. Boston, Richard G. Badger (c. 1916). 294 p.

Rational sex ethics. By W. F. ROBIE. Boston, Richard G. Badger (c. 1916). 356 p.

The psychoanalytic method. By OSKAR PFISTER. Authorized translation by Charles Rockwell Payne. New York, Moffat, Yard and Co., 1917. 588 p.

The neurotic constitution; outlines of a comparative individualistic psychology and psychotherapy. By ALFRED ADLER. Authorized English translation by Bernard Glueck and John E. Lind. New York, Moffat, Yard and Co., 1917. 456 p.

Contributions to psycho-analysis. By S. FERENCZI. Authorized translation by Ernest Jones. Boston, Richard G. Badger (c. 1916). 288 p.

Leonardo da Vinci; a psychosexual study of an infantile reminiscence. By SIGMUND FREUD. Translated by A. A. Brill. New York, Moffat, Yard and Co., 1916. 130 p.

Wit and its relation to the unconscious. By SIGMUND FREUD. Authorized English edition by A. A. Brill. New York, Moffat, Yard and Co., 1916. 388 p.

For a long time students of psychoanalysis had to read German, but within recent years we have now already a considerable and rapidly growing body of literature in English. The above works have appeared almost simultaneously, all of them in America.

Of these the most important is that of White, because he attempts to give a general epitome of the leading topics, addressed not specifically to physicians but to psychologists and the interested public. His work is divided into thirteen chapters. He first gives us a genetic approach to consciousness, with an excellent, though perhaps somewhat inadequate, discussion of that most vexed of all problems, the relations of the foreconscious to the unconscious. Then follow chapters on conflict, symbolism, dream mechanism, the family romance, two chapters on the will to power, extro- and intro-version, and the resolution of the conflict.

Another independent treatment, although far more elementary and inadequate, is the work of Bjerre, who goes back to Kant and then passes on to Wetterstrand and the Nancy School, then discussing psychoanalysis as a science and method of treatment, the Adler doctrine of neuroses, the nature of hypnosis, the conscious versus the unconscious, extracts from a case history, points of view, and the outlook. The attitude of the author of this book, while expressing great appreciation of Freud and giving considerable time to a popular exposition of his views, is on the whole unconverted and somewhat critical, magnifying, perhaps somewhat beyond bounds, the differences between the different expositors.

Robie's work perhaps hardly belongs in the Freudian literature. Nevertheless he believes in and has made extensive use of psychoanalysis, but his attitude remains somewhat independent. He treats sex matters with the greatest frankness and gives many cases of his own.

Of the translations, Payne's of Pfister's comprehensive work is by far the most important. Pfister is a young Zurich pastor, an ardent disciple of Freud, who has already published a number of interesting original studies of his own, and who here presents a sketch of the entire movement, epitomizing the views of its leaders, and even those of Adler and Jung, although his own sympathies are mainly with Freud.

The translation of Adler also meets a long-felt want because this work, although published several years ago in Germany, represents the great schism led by the author, who substitutes the horror of inferiority, the ambition to do something and be of importance in the world, for the sex theory of Freud. His manly protest, his doctrine of compensation, are invaluable new contributions not only to normal but to abnormal psychology.

Ferenczi is perhaps second only to Jung and Adler, among those inspired by Freud, in originality and independence, and here we have his most important contribution.

The two translations of Freud by Brill, who has already rendered us much service in this field, enable the English readers now, with the aid of his other translations, to come into almost first-hand contact with the founder of the new analytic school.

G. S. H.

BOOK NOTES

Psychic phenomena, science and immortality. By Henry Frank. Boston, Sherman, French and Company, 1916. 556 p.

This second edition contains a long preface, and as the book has not been reviewed in these pages, we give in the following the main topics treated. Book I, on psychic phenomena, considers some revolutionary scientific intimations, discoveries, the seat of the sub-conscious mind, the soul's secret scroll, psychic and physical correspondence, the physiological underworld, the mind's mysterious mirror, super-physical senses, Crookes' experiences, the subterranean self, invasion of personalities, the law of personal integrity, the sleepless self, the bond of psychic unity, memory, maker of personality, mechanical mechanism of memory, psychic phenomena and soul-substance, spirit-forms and materialisation. Book II is on scientific interpretation, here contrasted with explanations, and treats ultimate matter and vital energy, recent mysterious scientific discoveries, some occult forces in nature, the subtle seat of human intelligence, biology of the soul, scientific discovery of the soul-body, tentative explanations of psychic phenomena, thought and radio-activity, physical basis of telepathy, substantiality of thought. Book III is on the problem of immortality, and treats the scientific hypothesis of immortality, radio-active energy and immortality, with a summary of scientific arguments for it.

The philosophy of William James. By Th. Flournoy. Auth. tr. by Edwin B. Holt and William James, Jr. New York, Henry Holt and Company, 1917. 246 p.

James was asked in the spring of 1910 to address the Christian Association of Swiss Students at its meeting at Sainte-Croix and he consented provided his health permitted; but later had to give up the project and soon after came home and died August 26, 1910. Flournoy was asked to take the vacant place on the program and his discourse, amplified and revised, is here printed. An appendix contains his review of James' "Varieties of Religious Experience." The topics treated in these chapters are James' artistic temperament, early environment, rejection of monism, pragmatism, radical empiricism, pluralism, tychism, meliorism and moralism, theism, the will to believe, with a summary and conclusion.

The psychology of religion. By George Albert Coe. Chicago, University of Chicago Press, (c. 1916). 365 p.

This work is intended primarily as a handbook for beginners in the psychological analysis of religion. Its prime purpose is to make clear the problems, the kinds of data, the methods of research, and the achieved results. Its justification is partly the inherent difficulty in analyzing religious experience and partly the youth of the psychology of religion, a topic which is really just beginning. The topics are as follows: Religion as an object of psychological study; the psychology of mental mechanisms and of persons, the data and

how they are ascertained, preliminary analysis of religious consciousness, racial beginnings in religion, the genesis of the idea of God, religion and the religions, religion as group conduct, religion as individual conduct, conversion, mental traits of religious leaders, religion and the subconscious, the religious revaluation of values, religion as discovery, religion as social immediacy, mysticism, the future life as a psychological problem, prayer, the religious nature of man.

Creative intelligence; essays in the pragmatic attitude. By JOHN DEWEY and others. New York, Henry Holt and Company (c. 1917). 467 p.

This is an attempt at intellectual coöperation with no attempt at unanimity of belief and no effort to proffer a platform of "planks" on which there is agreement. It presents a unity in attitude rather than a uniformity in results. John Dewey writes on the need for a recovery of philosophy; Addison W. Moore, on reformation of logic; Harold Chapman Brown, on intelligence and mathematics; George H. Mead, on scientific method and individual thinker; Boyd H. Bode, on consciousness and psychology; Henry Waldgrave Stuart, on the phases of the economic interest; James Hayden Tufts, on the moral life and the construction of values and standards; Horace M. Kallen, on value and existence in philosophy, art, and religion.

The psychology of the great war. By G. LE BON. Translated by E. ANDREWS. New York, Macmillan Co., 1916. 480 p.

M. Le Bon here discourses in an easy popular style, and in the light of the psychological concepts which his previous works have made familiar to us, of the national mind, of Germany's recent development and of the psychological elements in her methods of warfare, of the remote and immediate causes of the war, of the psychological forces involved in battles, of the value of official reports, of peace problems. He has his own point of view, but is open-minded enough to admit that "a method [the German, to wit] which permits a nation to utilise the least brilliant brains in its midst is invaluable to it." Overpopulation and a desire to create markets are discounted as German motives to war-making; the puzzle of the Marne is discussed but not solved; the importance of Antwerp is stressed. A readable book written on the war by a psychologist, rather than a psychology of the war.

Health and disease; their determining factors. By ROGER I. LEE. Boston, Little, Brown and Company, 1917. 378 p.

This is a very comprehensive book, dealing with heredity, food, air, skin, exercise, tobacco, drugs, light and the eyes, teeth, hygiene of the mind and nerves, communicable diseases, those diseases transmitted by ingestion, air-borne diseases, those transmitted by contact, venereal disease and sex hygiene, the insect-borne diseases, diseases the method of spread of which is unknown, cancer, milk, water, sewage, occupational diseases, function of the board of health, and vital statistics.

The psychology of drawing. By FRED CARLETON AYER. Baltimore, Warwick and York, 1916. 186 p.

Part I, on the scope of the problem, outlines the problem, general and specific, gives definitions and general procedure. Part II surveys

the literature of drawing and outlines the methods of research and bibliographical survey, relation of drawing to the intellectual development, analysis of the drawing product and act. Part III gives experiments, representative drawings, drawing and school grades, analysis of observation during drawing. The work ends with conclusions and a bibliography. It embodies the results of a study of drawings as a device in laboratory teaching which consisted of a survey of the existing literature on the psychology of drawing, an effort to characterize the chief contributions, with a summary of results.

Principles and methods of teaching. By James Welton. Baltimore, Warwick and York, n. d. 2d ed. 677 p.

The purpose of this book is to help teachers in their daily work. Its fifteen chapters are entitled as follows: General function of teaching, material of instruction, form of instruction, teaching of English (with separate chapters on preparatory, reading, literature, composition and grammar, summary), teaching of music, of history, of geography, of natural history, of mathematics, of form, of needlework, with an appendix on the teaching of modern languages.

The doctrine of formal discipline in the light of experimental investigation. By Nellie P. Hewins. Baltimore, Warwick and York, 1916. 120 p.

The chief problems of educational psychology include the nature of mental endowment or the original nature of man and the nature of the learning process and of training. The last has developed recently much material and is of prime importance for educational theory, though its solution as yet is far from being obvious or simple. The problem of the transfer of training is especially unsettled. This work is divided into two parts; historical, with an account of investigators and means, methods, results and conclusions of experimenters in historical review; and original investigations, with conclusions rather concisely summarised.

The mentality of the criminal woman. By Jean Weidensall. Baltimore, Warwick and York, 1916. 332 p.

This investigation was the outgrowth of an earlier one, begun in 1911, under a grant from the New York Foundation. In its present form it has been carried on as one of the chief issues of the Laboratory of Social Hygiene. The experiments were focused on the possibility of securing a body of mental tests that could be applied after a woman's conviction and preceding her sentence and that would prove prophetic of her reformability. The volume contains very interesting data, tables and results, and constitutes a real contribution to the subject.

The experimental determination of mental discipline in school studies. By Harold Ordway Rugg. Baltimore, Warwick and York, 1916. 132 p.

This monograph demands attention (1) because it presents in compact semi-tabular form a comprehensive summary of all the experimental work done on formal discipline to date; and (2) because it presents the results of the author's own investigation, which is con-

spicuous because it deals with a large number of subjects (students in the University of Illinois), measuring effects of mental efficiency produced by a course of instruction in geometry. They demonstrate a certain degree of transfer of training.

The study of the behavior of an individual child. By JOHN T. MCMANIS. Baltimore, Warwick and York, 1916. 54 p.

This syllabus is the outgrowth of an attempt to direct prospective teachers in classes in education to understand child life in the city. It has been found more effective to study individual cases rather than the child as a type or children in general. It is a comprehensive and systematic direction for observation by those who have access to children.

A point scale for measuring mental ability. By ROBERT M. YERKES, JAMES W. BRIDGES, and ROSE S. HARDWICK. Baltimore, Warwick and York, 1915. 218 p.

This work is divided into five parts, as follows: constitution and relations of the point scale; results of the application of the scale to normal individuals; results of the application of the scale to defective or deranged individuals; revision of the scale; and the outlook.

The general value of visual sense training in children. By CHANG PING WANG. Baltimore, Warwick and York, 1916. 85 p.

The feature of this monograph lies in the use of school children as subjects and of sense training as a medium of experimentation. It throws valuable light upon the issues of the latter which is almost a fetish of the present popular Montessori method.

Studies in democracy. By JULIA H. GULLIVER. New York, G. P. Putnam's Sons, 1917. 98 p.

This book describes (1) the essence of democracy; (2) the twentieth century search for the Holy Grail; (3) the efficiency of democracy.

Mortality statistics, 1914. Fifteenth annual report. Department of Commerce, Bureau of the Census. Washington, Gov't. Printing Office, 1916. 714 p.

Zur Frage nach den Geschlechtsdifferenzen im akademischen Studium. By ANNA WISSE. (Reprinted from the Zeitschrift für angewandte Psychologie, Bd. XI, Heft 4/5.) p. 341-401.

The history of the psychoanalytic movement. By SIGMUND FREUD. (Nervous and Mental Disease Monograph Series, no. 25.) New York, 1917. 57 p.

A study of perseverance reactions in primates and rodents. By G. V. HAMILTON. (Behavior Monographs, vol. 3, no. 2, 1916.) Cambridge, Henry Holt and Company, 1916. 65 p.

Theodule Armand Ribot—1839-1916

Theodule Armand Ribot was born in Guingamp, in northern France, December 18, 1839. His early education was obtained in his native town and in Saint Brieux; but was interrupted at the age of seventeen years by the necessity of seeking employment. After six years spent in a clerical position he entered the École Normale Supérieur where after three years' study he graduated in 1865. He was professor of philosophy at the Lycée of Vesoul from 1865-68; and he occupied a similar position at Laval from 1868-72. In 1872 he removed to Paris where the next thirteen years of his life were devoted chiefly to the clinical study of mental abnormalities. In 1885 he was placed in charge of a course in experimental psychology at the Sorbonne; and in 1889 a chair of experimental and comparative psychology was created for him at the Collège de France. He established the *Revue philosophique* in 1876 and served as its editor until the time of his death, which occurred December 8, 1916. Professor Ribot was a Member of the Institute, and a Chevalier of the Legion of Honor.

Professor Ribot was a frequent contributor to the psychological and philosophical magazines of his own and foreign countries; and he translated Spencer's Principles of Psychology into French. He was the author of numerous books, many of which have been translated into English and other languages: *La psychologie anglaise contemporaine* (1870); *L'hérédité psychologique* (1873); *La philosophie de Schopenhauer* (1874); *La psychologie allemande contemporaine* (1879); *Les maladies de la mémoire* (1881); *Les maladies de la volonté* (1883); *Les maladies de la personnalité* (1885); *La psychologie de l'attention* (1889); *La psychologie des sentiments* (1896); *L'evolution des idées générales* (1897); *Essai sur l'imagination créatrice* (1900); *La logique des sentiments* (1905); *Essai sur les passions* (1907); *Problèmes de psychologie affective* (1910); *La vie inconsciente et les mouvements* (1914).

That Ribot's interests and his envisagement of the problems of psychology passed through a definite series of developmental stages is indicated by the titles and the chronological sequence of his writings. As he himself has pointed out, his earliest publications,—a survey of recent psychological movements in England and in Germany,—were undertaken for the purpose of ridding France of the incubus of the "elegant but empty theories" which were then current in his native land. Ignoring the polemics to which these two volumes gave rise, Ribot next devoted his energies to an attempt to approach the normal mechanism of mind from the pathological point of view; here belong his monographs on the abnormalities of memory, volition and personality. Within a few years, however, the necessity of offering courses on the higher mental processes compelled him,—much against his will, as he himself testifies,—to attack the more complex problems of attention, imagination, generalization and emotion; and it was to this group of problems that he devoted himself throughout the later years of his life.

The most distinctive features of Ribot's work in psychology resulted from his conviction that every mental phenomenon should be approached from the twofold point of view of its biological evolution and its morbid dissolution. Ribot's systematic work is characterized by a predilection for the unconscious (probably a heritage from Maudsley, Lewes and Schopenhauer) and especially for the organic and motor phenomena (probably a heritage from Bain). His writings indicate that he realized, more adequately than his predecessors, the significance of the affective and emotional phenomena in all of the activities of life.

J. W. BAIRD.

Joseph Jules Dejerine—1849-1917

The death is announced, at the age of sixty-seven years, of Professor Dejerine of Paris, well known in the general fields of psychiatry and neurology because of his many publications in both fields. He was for many years a physician at the important asylum of Salpêtrière, and was also Clinical Professor of Nervous Diseases of the Faculty of Medicine in Paris. Professor Dejerine was a member of the Société de Biologie and was its Vice-President in 1895; he was also a member of the Académie de Médecine. Professor Dejerine has left two monumental works: *Anatomie des centres nerveux,* in collaboration with Mme. Dejerine-Klumpke, the first volume of which appeared in 1895; and *Les manifestations fonctionnelles des psychonévroses, leur traitement par la psychothérapie* 1911, which was written in collaboration with E. Gauckler. Besides these two large works Dejerine has contributed many articles to the periodical literature. For example, over twenty articles on aphasia alone have appeared from his pen between the years 1879 and 1895. His really important contributions on the subject of aphasia are embodied in a few short communications to the Société de Biologie in the years 1891-1895. But Professor Dejerine's periodical contributions are by no means confined to the subject of aphasia, as he has published a great many exceedingly valuable papers concerned with a very wide range of topics in both the general fields of neurology and psychiatry.

S. W. FERNBERGER

Sir Edward Tylor

The services of Sir Edward Tylor, whose death at an advanced age is announced, deserve recognition on the part of psychologists. As is true in other instances, the actual germinal contributions to phases of interest now incorporated in psychology, were made by men outside that specialty; Helmholtz, the physiologist of brain functions, Charcot and the psychiatrists are cases in point. In much the same way Tylor laid the basis of a considerable section of social psychology. The psychology of primitive man, of the survival of primitive ways of thought in later ages, the interpretation of myth and custom, of language and art-products as massive psychic expression, owes much to his lucid and fascinating presentations. Wundt—who is of about the same age as Tylor—rounds out his remarkable career by a monumental work on "Folk Psychology," which is Tylor's theme in a different setting. The decisive attitudes toward the psychic product in the social mind, historically and in the present living form, which come so naturally to the twentieth century student, were in considerable measure first framed and effec-

tively launched in Tylor's "Primitive Culture" and his "Early History of Mankind." This obligation of psychology to a remarkable anthropologist it is a pleasure to record.

Tylor came directly under the influence of the evolutionary movement in its initial momentum, when the vista of the promised land invited entry. He contributed to the refashioning of the history of man in evolutionary terms. In the domain of psychology that task required the highest type of ability and a comprehensive power of expression. These were united in rare degree in Tylor's person. His was a commanding presence; and no one who heard him in the fulness of his powers will lose the impression of a remarkably vigorous and attractive personality. About twenty years ago he suffered from a stroke, from which he recovered though with impaired energies. Yet the work of his mind was adequate to continue his activities in restricted measure.

Sir Edward's fame rests upon his contributions to anthropology, but particularly to the psychological phases of that science. For this reason it is appropriate that a tribute of appreciation be recorded in the journals devoted to the professional interests of psychology.

JOSEPH JASTROW.

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FROM HOME TO THE CHARGE: A PSYCHOLOGICAL STUDY OF THE SOLDIER

By Charles Bird, Clark University

From Citizen to Soldier

The men who are fighting under the peculiar strain of modern war, exhibiting qualities apparently hitherto foreign to their nature, living in unprecedented situations and enduring hardships with great calm and utter fearlessness were a short time ago peaceful and normal citizens. To understand the transformation of character many personal narratives must be followed and close investigation made of the subtle influences which have played upon the soldier mind. Whatever conclusions are derived it is evident the soldier is created out of the citizen material, that with him he brings in some way not yet clearly defined the legacy of the phylum, the power of response to certain situations, and that according to the persistence of human experience the pre-war man must largely determine the responses made to the unusual environment of the battle-field. The evidence so far accumulated is sufficient to demonstrate that even the mental attitude assumed towards army service will often determine types of behavior under certain circumstances. A soldier is governed by his desires. If he has unwillingly renounced the past to participate in the ordeal of battle ensuing unconscious psychic conflicts play a great role in his new career. All such soldiers, whether volunteer or conscript, are tremendously influenced by repressed desires and the light thrown upon the working of the unconscious by a study of war neuroses opens up a large field for psychologists. What phylogeny and onto-

geny contribute to soldier qualities must be considered therefore along with the response made to the immediate environmental stimuli.

So suddenly precipitated was the present conflict that the potential soldier had very little time for reflection. The nation was plunged into mental chaos. Previous to the declaration of war this was characterized by psychic tension—expectation, restlessness, anxiety and a growing depression. Responsible individuals tried to comprehend the pending national catastrophe from a personal standpoint; political considerations were intensified and the ego assumed greater importance as these were finally interpreted in the light of protection for family and of business interests. When hostilities began however, there resulted a submerging of the individual into the national consciousness, a process which was slow and never complete in the experience of many. Life was characterized by quick associations, impulsive and precipitate judgments and greatly accelerated activities. Individual thinking almost ceased, the processes of consciousness for many were stagnant and dissociated; people were governed almost entirely by their emotions. All minor political and religious differences faded, the sense of self was diminished and persons who were previously reserved and self-contained found themselvs one with thousands of others in will, acts and feelings. In this condition where there was marked contagion of affective states and imitation played an unusually important rôle people often acted with abandon and regretted their behavior in the few quieter moments of reflection. Everywhere the mob spirit gained mastery. Those who as individuals were respected and admired for mental stability and independent action were one with the crowd in thought and deed. All psychic activity was collective, strong and quick. There seemed to be a diminution of personal responsibility, of the ego's power to resist erstwhile repugnant and antipathetic suggestion. Consequently unnecessary food panics resulted and the least item of bad news assumed alarming proportions. In such a state of unbalanced mental equilibrium communities reacted all out of proportion to the nature of the stimuli, and some collapsed under the pressure of the feverish activity. Owing to business panics men were forced into unemployment or else were requested to enlist immediately if they desired to retain their position in the post-war period. Whole cities faced new problems through the influx of soldiery and the commmandeering of equipment necessary for government purposes. In a few days the tra-

ditional, habitual modes of living experienced a metamorphosis bordering upon the miraculous so that people lived in an unreal world, often of overwrought fancy.

Great credulity was characteristic of the peoples of all the warring nations. Thus in Germany stories of the capture of the Russian fleet in the Kiel Canal were believed and one author, (W. D. Kurt), tells of a persistent rumor that a yellow French auto with 400 millions in gold was somewhere on its way through Germany to Russia and all kinds of precautions were taken. Bridges were barred and watchmen were posted. A driver of a large yellow auto was shot, apparently by the police, and many others were held up by officials and mobs. In one case even a Prussian officer on a suspicious auto was fired upon despite his vigorous protest. Rumors often reached the newspapers of the approach of a vehicle of this kind. In England we were constantly informed of the landing of Russian troops on the east coast of Scotland and that they were continually passing along the east coast on their way to France. Many persons vowed they had seen them even in the terminal station of Liverpool although as we now know such accounts were pure fiction. So complete was the mental instability that any story was immediately believed if it revived or bolstered the lagging and dejected spirits. The constant fear of danger to one's person and nation created psychic states voracious for fanciful phenomena.

In addition to the preceding factors the potential soldier was influenced by a more subtle suggestion. He saw the military uniform everywhere. In the parks he watched hundreds of young men drilling; men who had immediately enlisted. Their untidy appearance and awkward movements at first amused him, but soon, as the men under competent leaders gained poise and military smartness, his contempt turned to respect. Invariably he experienced strange emotions followed by the decision "I must enlist." If he approached a recruiting station the long lines of young men eagerly and patiently waiting to join mysteriously attracted him. Many have declared "The mob spirit gained mastery over me." An important suggestive factor was the advertising posters. Wherever the citizen turned he was confronted by them. They were appealing. He was hypnotized by their personal application. He was needed; upon him depended the safety of his country and his home. At one moment his social instinct surged into consciousness, another that for self-preservation. Furthermore these posters created group attitudes until everyone thought in the mode they sug-

gested. They became topics of conversation with a moral of no uncertain sound, and thousands responded at their suggestion. While the picture was the most appealing factor of these posters, the short, pointed sentence, printed in appropriate ink, seldom failed to occupy the attention for hours and days. As an illustration of these we have the following:

1. "England Expects Every Man to do His Duty"
and
Join the Army
To-day.

2. "Join the brave throng that goes marching along." (with picture of soldiers marching).

3. Surely you will fight for your King (picture of) and Country (picture of). Come along boys before it is too late.

4. Why Aren't
You
in Khaki?
You'll be wanted.
Enlist at once.

5. Thousands
Have Answered
The Nation's Call,
But You May Be
The One
To turn the scale
at a critical moment.

Do you realize this?

Consequently from the factory, work-shop, store, farm, office and college, men flocked to the colors. They had been motivated to respond by many forces which may be summarized as: the emotions of fear, anger, and the instinct of self-preservation; love of adventure; social pressure including family tradition; economic pressure; the desire to win the praise of friends and of the community, and the sense of the heroic. Undoubtedly for some the army offered a path of least resistance, a means of security with approval, so uncertain had personal matters become. Of this heterogeneous army Von Hindenburg said, "Even if there were a million, they would form no real army, but a uniformed crowd." He did not estimate the fundamental nature of the

human organism and its power of adaptability when activated by the instinct of self-preservation, nor did he consider the moulding force of a directed purposeful environment.

The army is a leveller of social groups. In the ranks common dangers, sharing of difficulties, eating, sleeping and working together, wearing similar uniforms and being subject to the same rigid rules, weaves around the men a cloak of brotherhood and breaks down individual distinctions. Between men and officers however there remains a social distinction. It appears necessary to enforce discipline. Men will more readily obey those whom they are accustomed to recognize as socially superior, who have "blue blood" in their veins. Even older men readily obey the commands and trust themselves to younger officers provided they possess the mark of social superiority. When once the average soldier dons the uniform his individuality begins to submerge, to dwarf into nothingness and to find a new life in the group consciousness. One man said, "We had lost our individuality, and it was to be months before we regained it in a new aspect, a collective individuality of which we became increasingly proud." (27, p. 9.)

Personal liberty becomes a thing of the past; the new society and the state assume paramount importance. With this transition there comes peace, mental poise and relaxation. Previously the emotions had been at a high tension, men felt as if they would 'burst' as they were torn asunder by impulses and desires. Now with the loss of responsibility they can rest with abandon, cease to think about old problems and find psychic and physical relief in the simplicity of their surroundings.

The rapidity of adaptation is determined by the mental constitution of the individual and the motive which prompted his enlisting. If he unreservedly entered the army, or has reconciled personal and state obligations, he will soon fit in; but if not, if he has been coerced into army life, his actions will be marked by sullen independence and unsocial behavior, while probably some mental complex will be built up to buttress his desires and to obtain release from actual fighting.

Necessarily the change is slow. After six months training in Canada and England a college friend wrote, "It seems strange not to be able to arrange one's own plans but to merely obey and to shut up," and another soldier put it, "At the beginning they were individuals, no more cohesive than so many grains of wet sand. After nine months of training they acted as a unit, obeying orders with that instinctive

promptness of action which is so essential on the field of battle when men think scarcely at all." (27, p. 32.)

No environmental change could be more radical. New forces impinge upon the mind and create a different mental attitude. Everywhere things move with precision, organization makes life mechanistic, and the details of the present control the attention and blur the memory of the past. Initiative and freedom of thought are no longer possible. Both are dangerous. The men must obey. Consequently discipline is all-important and to secure the psychic state conducive to child-like obedience and unquestioning performance of commands, and at the same time to keep the respect of the men, is the most difficult task of the officer.

"Half the work of their instructors consists in getting them into the proper frame of mind and giving them that esprit de corps which is essential to the war fitness of a volunteer army." (67, p. 11.)

At first the men are surly and obey unwillingly. They find fault with details yet accept hardships cheerily. The details appear as impositions seemingly unnecessary from their point of view of war. Gradually they realize the futility of kicking against the pricks and learn they belong entirely to the government. Only the officers are allowed to think upon the affairs of the regiment. It is a dogma that they alone possess common sense.

"To safeguard this dogma from ridicule it is necessary that the men should be prevented from thinking. Their attention is to be fully occupied with such mechanical operations as the polishing of their buttons in order that the officer may think without fear of contradiction." (28, p. 31.)

and

"Cleaning greasy pots, scrubbing floors and drilling produced no thrills. They simply bored us. Life was dull and prosaic, and, as we have said, uncomfortable. No one ever said anything interesting. We never got a chance to sit down and think things out." (28, p. 107.)

It should be noted that the author of the preceding quotations represents an extreme type of individual. Being a university graduate he naturally finds army life dull and prosaic, yet he is better equipped to pass judgment upon the mental lethargy so common to the soldier experience.

In the training camp, hundreds of miles away from the war zone where life is simple and strict, the finer sensibilities are clouded, the mind assumes little importance and the body occupies the prominent place. Men live in the realm of the physical; the psychic activities are reduced to the lowest level. There is no mental stimulation, intellectual compan-

ionship is rare, the attention is focussed upon the immediate surroundings and conversation is concerned with the trivialities common to the camp. Every man thinks about the same subjects, which are seldom philosophical, religious, emotional or problematical; there is lack of criticism, but great docility and naïveté so that the mind becomes simple, shallow and childish. Very seldom do they talk about the trend of the war, yet they look forward eagerly to their advent into the war zone. Gradually images of the civil life fade and only in a limited manner do they think of home and friends. Their quarrels are usually over the possession of pleasure-supplying commodities; they never suffer mentally, only physically. Some men are exceptions owing to their previous mental training but by far the majority live entirely in the realm of sense impressions.

For the most part the soldiers who are in the ranks represent a society unaccustomed to specific intellectual pursuits; they have been accustomed to obey their superiors in other careers and consequently the surrendering of individuality is not difficult and is, in fact, compensated by the pleasures and physical activities of the group. There ensues however for a smaller company of men of intellectual attainment a mental conflict in which the love of individual liberty and self assertion, long enjoyed and cherished, revolts against military despotism. Owing to the rigorous discipline and the absence of mental stimulation, to the sense of isolation, these men are forced to repress their wishes and to philosophically accept the fate of their environment. Some yield to the regressive tendencies and with the majority find pleasure on the lower phyletic levels, but others retain their former poise and personality, they have a strong imagination or as one has aptly expressed, a sense of the dramatic. They obey and execute commands immediately and often become leaders of companies but they are never really one with the crowd in thinking and behavior. Representative of this group are business men and students. One of these says:

"It is a faculty which gives zest to life: putting boredom and oppression to flight, stimulating humor, humility and idealism." (28, p. 182.)

Few, however, can sublimate their wishes and turn their energy into imaginative and constructive channels. The majority revert to animal levels, are the slaves of impulse and desire and are governed almost solely by the pleasure-pain principle. Much of their spare time is spent telling obscene stories and in jesting about sexual matters.

A soldier's training aims to give a consciousness of physical fitness. Nothing is more important than a sound body, well coördinated, supple, healthy and strong. It increases self-confidence, gives self-control, creates morale and prepares the nervous system for the more strenuous work of actual fighting. Also certain types of behavior must be established, the mind accustomed to resist shock and a peculiar emotional experience evoked, similar to the excitement of the charge. All activities are planned to elevate the physical and to put the intellectual life in abeyance. From reveille until "lights out," the red blood is kept pounding through their veins. At daybreak the men are awake and for one hour before breakfast go through Swedish drill. As they progress in the execution of duties, leaving behind squad, platoon and company drill, the more important work of field manoeuvers constitute part of the program. Twice a week the battalion marches from ten to fifteen miles. For months several hours daily are devoted to musket and bayonet practice. During the latter the men charge over fields and into the trenches to thrust the blade into dummy figures. Trenches are constructed, instruction is given in bombing, sniping and the use of machine guns, in fact, every condition of the war zone is imitated as much as possible. Special training is afforded according to the division and type of service, and all is characterized by earnest, eager endeavor to do the task thoroughly and creditably for the honor of the battalion. Occasionally mimic battles take place in which between ten and twenty thousand men on each side oppose each other. Artillery, infantry, cavalry, field ambulance, air craft, in fact, every branch of army service engage and share in victory or defeat. In all practice of attack in open formation upon intrenched positions the men finish with terrific bayonet charges shouting lustily as they proceed.

After the duties of the day are over the soldiers engage in boxing, wrestling and athletic contests. The spirit of rivalry, of attack and defense is fostered whenever possible. Companies compete with one another in all branches of athletics: the soldier is a sportsman. In many ways the physical energy finds expression. There is continual horseplay, much practical joking, very often fist fighting, and withal much pleasantry, and an abundance of fun. Various organizations, foremost of which is the Y. M. C. A., provide entertainments and social games, by far the most popular being those in which the men can participate either by singing or performing. As a rule all are physically tired when 'lights

out' is sounded at nine o'clock; they are contented and feel as never before radiantly, buoyantly healthy.

When the necessary adjustments are made to the new conditions of the camp, life does not appear monotonous and dull. Time passes very rapidly and many prefer the varied program to the old forms of labor in the workshop, factory, on the farm, or at the office desk. Great pleasure, exhilaration and satisfaction are derived from the war game. On the march the rhythmical swinging step, martial music, admiration of the onlookers and the glamor of the uniform evoke a consciousness of strength, increase the feelings of contentment, elation and pride and simultaneously help to subordinate the ego to the welfare of the battalion. Participation in manoeuvers mechanizes the mind, develops the power of self-control, enforces discipline, curbs curiosity, suppresses fear and eliminates the use of initiative unless the soldier is a part of a special squad assigned to such practices as bombing, sniping and scouting. Fatigue duties, caring for the camp and attending to the many details of military life submerges individuality, narrows the mental vision, fosters patience, discipline, and respect for their officers. After months of training the men grow tired of the war game and become restless, eager and desirous to go abroad.

A natural question is: By what method are the men located in the specific branches of the army service? How are mental traits and physical aptitudes correlated with the new sphere of activities?

To such questions we can only answer we do not know. That some process of analysis takes place is certain but the actual procedure is a government secret. Some French morphologists (10) have suggested that their government bases the assignments upon the type characteristics. These types are conditioned by rapport with the environment, cosmic elements, air, earth, and water. The examinations consist of measurements of the head, the trunk and the limbs. Four types are suggested: the muscular; digestive; respiratory and the cerebral, but more often combinations of these exist. As a result of continued struggle against the environment we have the muscular type. He is characterized by a rectangular shaped face, and strong, regular, well proportioned limbs. This type constitutes 47% of the French people and is found in the agricultural districts. As a soldier he is best in attack, or if muscular-digestive he makes a good artilleryman. The digestive type has a truncated-pyramidal shaped head, a large digestive apparatus, the trunk is long, the limbs short,

round and fat, and the thorax is small. He depends upon good alimentation for his well being. As a soldier he is best in defense. The third type, respiratory, supposedly a descendant of nomadic tribes, has a large thorax and the face is hexagonal shaped. Respiratory individuals are best fitted for service in outposts, in high altitudes including aeronautics; they make good sentinels and cavalrymen. Finally, the cerebral, smaller in figure, with a face shaped the reverse of the digestive and often brachycephalic, supplied before the war only 7% of the number of soldiers. This type suffers least from hardship at the time, keeps up in forced marches, and makes good officers and secretaries. The disposition to disease of these divisions may be generalized as (1) catarrh and asthma; (2) disease of the alimentary apparatus; (3) rheumatism; and (4) headaches. Naturally pure types are rare but usually there is a predominance of one over another. Probably such a system has been adopted by the French and applied in the selection of men during the present conflict and that most of the larger nations have made a selection based upon certain mental and physical dispositions is an established fact.

To this interesting problem the camp life of the soldier lends some suggestions. In connection with every site there are numerous things requiring care and attention. Miniature cities are erected in a few days and when completed they need repairing. So we find men are assigned duties which approximate their vocations. The laborers dig roads and install drainage systems, joiners erect huts and attend to all wooden structures, electricians contribute their experience, clerks are busy at headquarters sorting mail and assisting with the tremendous business of accounting and provisioning, cooks and bakers find their trade always needful and in many ways we see vocational selection. It does not stop here however. The miner becomes a sapper, the mechanic, usually of strong physique and quick mind, mans the artillery or becomes a machine gunner, the engineer builds bridges and cares for the unlimited mechanical appliances used in modern war, and the young business man is always useful in the headquarters division where brains and clerical capacities are required. Vocational selection provides men for special branches of service. It insures not only the necessary psychic qualities but the facility of execution which can only come from long acquaintance in the particular field. For still more specialized work such as the air service recourse must be taken to the finer type distinctions previously suggested.

One value of specialization is often overlooked. Not only does it guarantee organization and efficiency, but these in turn become forces moulding the soldier mind, fostering precision of movement, discipline, love of accuracy, and sociality. It produces the spirit of the hive. All are workers, the men perform their duties with zest and effort and obtain fun and pleasure from them. The exceptions to this are found amongst thinking individuals who are only bored by the monotonous repetition of manual labor. Withal the soldier endures hardships with serenity, patience and determination, grumbles at trifles, yet is ever alert to express his abundance of humor, gaiety and jocularity. When he leaves camp for the fighting front he has a strong physique, nerves of steel, a mind trained to obey and to coöperate, and inured to the dreariness of repetitive action and capable of finding relaxation in the meanest environment.

In this transitional period the tremendous moulding force of epigenetic factors is demonstrated. Everywhere the mind is impressed and altered by them. Other forces are also at work. The real self of the men, the unconscious, also motivates and directs them. It is not so much repressed by traditions and customs. Everywhere the sexual desires find expression resulting in a regression to the lower phyletic levels where coarseness and vulgarity vie with animalism. Perhaps this is due in part to the concentration of large numbers of men, who in society passed unnoticed in the performance of sexual perversions, but, now finding the restrictions removed, and lacking almost entirely the elevating moral influences of family and social life, are governed by this instinct, are controlled by powerful desire. The unconscious, the product of phylogeny and ontogeny finds in a soldier's environment outlets for long repressed wishes and thus reversionary forms of activity are manifested. On the other hand the love of life, and the impending fear of death build up mental complexes and effect changes which lie hidden until a crisis is reached. Later, in section four, we must view these from another aspect of soldier psychology, namely, the war neuroses. At the present, indications point to both epigenetic and endopsychic factors coöperating in producing the soldier out of citizen materials.

The Soldier in War

There is little to be written about the departure from the training camps, probably because the time has not arrived for men to recall such an apparently unimportant phase of war,

but when the period comes for reflection we shall undoubtedly learn more of the psychic states typical of what is really a momentous event. We know however a few details of this period. There are no parting scenes, no farewells; these have all been enacted weeks before upon the occasion of the last furlough. The men entrain for an unknown seaport and a still more remote destination. Only the cheers of comrades awaiting the next transport greet the men for all civilians are held in the rear of the quays. The business of war is early impressed upon the soldier mind, and as a business undesirable and to be quickly finished he will always regard it.

Although the few accounts we have of the journey from England to France all emphasize the cheeriness, jocularity and good humor of the men, there is little doubt that the experience of the soldiers is totally at variance with their apparent indifference to hardships. The surface hilarity may exemplify ambivalence, or it may result from a conscious effort to repress the expression of discontent and to banish the feelings of uncertainty which arise during this period of transition. On the troopship they are huddled together, obliged to sleep on the sawdust covered floor or on the side benches, while many are sea-sick and unable to rest. For the most part they do not betray their feelings by grumbling and complaining, rather they give the impression of contentedness as they sing songs, engage in banter, and congregate in groups puffing at their cigarettes and pipes. It is essential however that we notice the real mental attitude beneath a feigned conduct, for soldiers are human; they appreciate comfort although they do not anticipate much of it and often their good humor is simply a way of making the best of a bad job. Their conduct does not betray despondency; to many the occasion marks their first voyage or sight of the sea, they are filled with wonder and often delighted, but as an undercurrent to the surface thoughts, welling into consciousness in reflective moments, forceful and commanding, are the memories of home, and the homeland, combined with the imaginations of the unknown and long anticipated entrance into war. Of these reflections they seldom speak so that one notices only the customary cheerfulness which has been recognized as a soldier characteristic. Upon arrival at the port, they proceed to camp, a city of bell tents, where they procure equipment and rest for the night. Here men invalided from the firing line relate stories of battle and often leave the impression that it is one great adventure. Early the next morning they fill the long lines of troop trains which are usually composed of

trucks, very uninviting, uncomfortable and not conducive to evoke good feelings or pleasantry. The men undoubtedly endeavor to accept stoically these conditions and in this they are aided by the novelty of a new country, but lest any should confuse the happiness born of an advantageous adjustment to one's environment with the good humor and gaiety of the soldiers, evoked to soften the hardships of their experience, it is only necessary to point to the accounts of discontent often hidden beneath the rhetorical skill of an author. In a personal letter written immediately upon arrival "behind the lines" the case seems to be clearly defined:

"We were five days on our journey which was comprised of a combination of experiences I am not anxious by any means to encounter again, but still if they come—they come, and the best thing is to get through them as cheerfully as possible, which is perhaps not as cheerful, at times, as optimists in songs depict. We had a railway journey of twenty-eight hours, which was the most unique and luxurious I have ever had—a real joy ride. (I don't think). However we prepare for other things instead of a 'feather bed' existence and there are thousands much worse off than we are." (Written March, 1917.)

The cantonments, in which further training is given, vary in character according to their location and size. Invariably they are within sound of the guns where the men may accustom themselves to the minor conditions of the war zone. Usually they present a highly socialized community much like the early training camps. The discipline is now severe. Offences previously considered trivial are crimes punishable by confinement in the guard room. In place of practice in trench duties and longer periods of rest more rigorous tasks are enforced. There are endless parades and the customary practice of Swedish Drills, bomb throwing and bayonet fighting. According to one writer (27, p. 133) the men charged at dummy figures clad in the uniforms of German foot soldiers. If we apply the typical conditions of their early training to the cantonment and remember the dominating influence of the physical life, the narrowing of the psyche, the absolute dependence upon officers, the rigorous discipline, and the abundance of fun and horse-play, we can consider briefly some new factors which play upon the soldier mind.

The new arrivals are much impressed by the indifference to the actual struggle. Men who have been in the trenches seldom talk spontaneously about it, in fact, were it not for the boom of the guns and the incessant movement of troops one might suppose no war was proceeding. "Everything around here, except for the sound of the guns, appears so peaceful that one cannot imagine anything serious going on

about three or four miles away." This seems a typical attitude of the British soldier. Even before the present conditions of trench warfare, which might repress the desire to relate experiences, reticence regarding combat was very noticeable. As an illustration the words of one who lived close to the battlefield may be cited:

"It suddenly occurred to me, as we chatted and laughed, that all the time the English were here they had never once talked battles. Not one of the Tommies had mentioned the fighting. We had talked of home, of the girls they had left behind them, of the French children whom the English loved, of the country, its customs, its people, their courage and kindness, but not one had told me a battle story of any kind, and I had not once thought of opening the subject." (1, p. 167)

War is a business to be seriously left alone when the fighting hours are over. However, everywhere the men refer to personal safety and among the majority a passing remark embodies the wish for a "Blighty" wound which is a wound slight in character necessitating removal to a hospital in England. Another factor which the cantonment presents is the absolute confidence and calm of the men who have spent months in the trenches. They have tested the enemy's strength, repelled his attacks, experienced and survived many dangers and through all have developed qualities which cast out fear, so that what they have once accomplished they feel they can do again. A comparison of the members of an old with a new battalion while on parade previously to entering the trenches reveals the former as resolute, firm, composed and confident in their own strength, while the latter are restless, uneasy, tremulous and fearful. These three factors, indifference to war, the continual reference to "Blighty" wounds as insignificant and desirable, and the self-confidence of the veteran, are potent influences which greatly decrease the nervous tension, allay many fears and rid the battle-field of much of its terror for the recruits; although only actual experience produces the attitude which typifies the accustomed fighter. In addition to these may be considered the example of the peasants who are often under fire and are indifferent to it.

To return to the billet for a further glimpse of the activities we may say that life is varied, often severe and difficult, yet sometimes conducive to merriment and fun, when games are played, and theatricals, concerts and various forms of entertainments are organized in which the soldiers play the important rôle. The old form of story-telling has been superseded by the literary and humorous productions of mem-

bers of the battalions. Carrington has given an interesting account of some of these papers. He says:

"The French particularly have excelled in this. For example, they have issued a periodical in the Champagne, entitled Le Poilu, which defines itself as "A journal, humorous, literary, and artistic, of the life of the troglodytes; to appear when and where it can." It contains impressions of the war, messages from home, news and bulletins, Rabelaisian sonnets and other material. Another entitled La Gazette des Tranchées (issued in the Argonne), 'an organ founded to maintain the spirit of mirth in France' gives scraps of Parisian life, of the Boulevards, etc., in the character of a general 'Revue'." (9, pp. 54-55)

In contrast with the lighter vein there are the periods of religious observances, church parades and tent meetings, but a consideration of the soldier's religious attitude must be deferred until later.

Undoubtedly many other features not enumerated prepare the men for the actual combat. All the soldiers do not experience the intense coöperative life in the cantonment but live in old buildings, barns, or with the peasants. They do nevertheless participate in all other regimental observances, meet tested and hardened warriors, become acquainted with the boom of the guns, the sight of mud-covered comrades and in general pass through a stage of psychic degeneration, at least of the finer sensibilities, while confidence is gained to enter upon the older racial types of behavior which characterize trench warfare.

The first experience in the trenches is designed to introduce the new regiments to their task. They spend only twenty-four hours in them upon this occasion and are usually situated between well trained troops. The period has been named the "Parapet-etic School." As they move forward, experienced fighters, cognizant of the fear which possesses these students in warfare, never miss a chance to express jokes such as to casually inform them they "have been fattened for the slaughter," and "the communicating trench leads to the cemetery." Typical of soldier discipline incoming troops always give the right of way to the tired outgoing regiment. New troops are always reckless, eager to see the enemy, are slaves of uncurbed curiosity, and only learn at the cost of lives to keep beneath the parapet. The sentinels imagine foes lurking on "No man's land" and at the least disturbance the whole company will man the loopholes and fire recklessly into space. The slightly wounded relate their experience; everyone is intensely excited; and the officers are anxious, grave and worried lest their men should fail. When relieved all feel they have repelled attacks and deserve praise

for their conduct. The older soldiers say the newcomers use more ammunition and material in one day than they would in a month.

One of the first impressions upon entering the trenches is of the tremendous noise and incessant movement. There is little calm, or rest, for the brain. Artillerymen, machine gunners, snipers, sappers, airmen, all vie with each other to secure the coveted advantage called "morale." At first the mind is continually excited and overwrought.

"It must be true of this war (I hear the same thing on all hands) that what strikes you most about it is its overwhelming noise. All the other sensations are secondary—heat, cold, pain, fatigue, danger. That's Nature all over. She doesn't allow you to experience more than one emotion at a time. I know that the four days I've just spent in the trenches have left this one impression on my mind: a shattering roar which no language can describe." (71, p. 39)

Eventually the human organism adjusts itself to this ordeal, for the soldiers take pleasure in counting the shells, judging their size, following their courses and guessing where they will fall. According to their size and speed they have been given special names, such as "Coal Boxes," "Whistling Willies," "Jack Johnsons," or "Black Marias."

For days the walls of the trench, the sandbags, the inside of their dugouts and an occasional glimpse through a loophole or periscope at the German parapet, perhaps a hundred yards beyond, constitutes the visual range. Some men have been fighting an enemy for months without ever seeing him. Under these conditions there is a complete narrowing of the psychic life. The physical sensuous nature almost entirely rules. Constant exposure to danger, the focussing of attention upon the means of defense and self-preservation, the uncertainty of physical safety, lessens interest in the details of the day and produces almost a strange hypnotic state of mind in which physical utilities assume supreme importance. As a natural sequence food is very essential for the maintenance of combative efficiency. It has been said "The soldier before dinner and the soldier after dinner are two entirely different beings." The hungry ill-fed soldier cannot fight, he will grumble, lose confidence and often revolt, but given abundant food, with his tobacco and rum, he is an animal, ferocious in the fight and indifferent to the things of the intellect. Referring to such pleasures of sensory appreciations Sir Bampfylde Fuller writes:

"He (man) has so elaborated the simple enjoyment of eating and drinking that for many persons it becomes life's chief attraction; they live to eat; and it is interesting to observe how this pleasure dominates

all other interests when men relapse into the savagery of war. To the soldier in the trenches his meals become the pivot on which the day turns; an army must be well fed in order to respond cheerfully to the general's orders, unless, indeed, the men are fired by religious or patriotic ideals into a glow of self-sacrificing fervour. In conditions of long drawn war, men's tastes revert to the simplest forms and (as letters from the front have amply testified) the greatest hardships may be alleviated by presents of cakes and sweetmeats." (21, p. 139)

In times of intense fighting when for days it is impossible to procure rations the dead are stripped of their packs and one writer (45) reports a case where food covered with blood has been ravenously devoured. The latter should be taken with reserve. Every indication points to a reversion to primitive types, to lower racial levels where the needs of the organism are supplied no matter what the cost. When regiments are seated around the tables in billets after a battle, they notice the gaps in their ranks, and in a dim, almost unconscious way realize in some home there will be a measure of desolation, yet these gaps are taken as a part of war; by the laws of chance they themselves still eat and drink and can be merry. In the act of eating the pangs of sorrow entirely disappear. Food is theirs; why worry about what happened a few days ago. So it is that losses are not felt keenly.

On the firing line, the nearness of danger and possibly of death, although the soldier never believes he will be killed but by some miracle will receive merely a slight wound, automatically breaks down most of the individuality that might be left, throws the men more completely upon the resourcefulness of their officers, more closely unifies and welds the group together and insures complete obedience to authority. Men and officers are inseparably united by the common bond of danger, while their proximity reciprocally dispels the pangs of fear and uncertainty. The massing together of men governed by the instinct of self-preservation, intent upon the destruction of an enemy and performing onerous duties as effectively as possible, releases hidden forces of resourcefulness, endurance, and strength which are directed so as to protect not merely the individual but the group. All are bound together in a common cause; class distinction, social superiority, is forgotten among privates and even the officers are not regarded primarily as a socially privileged class but are superior because they possess authority, valuable information and are invested with the right to think and decide matters of extreme importance. Instinctively the need of a leader is felt; it is a soldier trait to imitate and this satisfac-

tion he obtains through his officers. Without leadership he knows the company would be a mob, ungovernable, and more dangerous than his greatest enemy. Fortunately the officers are usually men possessing most of the soldier virtues, who exemplify the valor, bravery, courage and recklessness necessary for aggressive warfare while excelling also in the important qualities of patience, determination, sociality, discretion and strong sympathy, so essential during the trying months of trench confinement. The graphic words of one of these men reveals a chapter of social psychology: " It's hard marching, shrapnel and machine gun fire, barbed wire and all the rest of it that levels cabbages and kings." (71, p. 28.)

Never before in army life has there been established between officers and men relations so cordial, often amounting to devotion to each other despite the very apparent differences of education and social advantages. Cases are by no means rare of supreme sacrifices, made, either to save their officer, or by an officer to protect his men. An officer writes:

" I've seen them wounded and keeping on as if nothing had happened. It's not that they don't feel the pain—make no mistake about that—they won't feel it. During an advance, if cover's scanty, they won't monopolize it if an officer is anywhere about. When you do the smallest thing for their comfort or convenience they're quick to appreciate it; in success their enthusiasm is the most sanely delightful thing conceivable; a reverse doesn't dishearten them. They grumble at trifles and laugh at difficulties. Oh! Tommy's a wonderful chap . . . . There's something about him, dirty or clean, swearing or silent, glad or sad, hurt or whole, that is just unbeatable. You can't knock him out. If our fellows are devoted to us it's only a case of reciprocation." (71, p. 53.)

Like gregarious animals whose strength increases in direct proportion to the solidarity of the group and whose fear vanishes with the feeling of unity so men in battle who live in a manner typical of the lower racial levels automatically seek the protection of comrades by sinking their personality into the fighting unit.

Balck. " In the danger zone which suddenly surrounds and startles him in war, the soldier feels in the first place, a desire to have someone assure him that the seemingly critical situation in which he finds himself is as it should be. His eye is naturally directed upon his officers. If the officer's quiet glance reminds him that here, as in peace time, the first duty is obedience, and if he subsequently sees the officer advance fearlessly and vigorously he will as a rule not worry about the why or the wherefore." (18, p. 33.)

Obviously dependence upon officers and fellowmen and the general condition of trench life fulfils most of the requirements of normal suggestibility. As a gregarious animal the soldier acts upon whatever is suggested to him. The

moment he enters the firing line he encounters an environment which fixates his attention upon one subject,—to kill his enemy and to preserve himself; his mind is distracted from all else and after the novelty of the first few days wears off, the monotony of his confined life narrows his mental vision and also limits voluntary movement. Thus tremendous psychic tension results; the strain of watching and seeing nothing becomes almost unbearable, and the continual emotions of fear which at first possesses all as a result of unseen foes combine to lower the reaction threshold to suggestion. The result of this is twofold. It magnifies the importance of the officers, giving to them prestige, and it merges the individual still further into the social consciousness, making discipline the handmaiden of self-preservation, and initiative its enemy.

Unless the soldiers became indifferent to the noise, to unceasing movement, to the squalid, muddy, narrow trenches, to cold, rain, heat, pain, and even death, life would be intolerable, impossible after the first few weeks' experience. It is an inherent quality of the human species to rapidly adapt the organism to its environment, and such an adaptation occurs. In order to more readily understand what is now recognized as almost absolute indifference to trench warfare, and to appreciate the genuine cheerfulness and happiness of the combatant in face of death and amidst scenes which would be gruesome, repulsive and detrimental to the strongest nerved civilian, it is necessary to determine the relative importance of fear, and also the soldier's attitude to death itself. Very often the statement has been made that the seasoned soldier does not experience fear, at least not in the front line trenches. This is only a half truth. The veteran both experiences fear and more readily confesses it than those less habituated to war, but he has gradually grown accustomed to the common dangers. For weeks the men suffer from intense fright as comrades are killed or horribly mutilated, and it is only gradually they overcome their fears and grow callous, indifferent, fatalistic—almost unmoved by the cries of wounded and the loss of friends. Fear, the primary emotion, is characterized by an ambivalent tendency. The typical forms of behavior which are its accompaniments may pass over into valor and courage, and the individual may radiate hope and cheerfulness. "Familiarity breeds contempt" or what is perhaps more true is that the human organism adapts itself to every situation, provided sufficient time elapses between the first stages of fright, and fear of the unseen and unpre-

ventable, and the secondary periods of familiarity with danger. It is not that the soldier does not fear but he has ceased to fear certain situations because he has been safe comparatively during his experiences, or he has escaped injury so often. Death is the negation of all his desires and he fears it except when he is activated solely by his emotions, then life or death do not seem to be in his keeping. The constant hope for a "Blighty" wound is born of a fear of death and so real is this fear that it creates compensating psychic states which culminate in a belief that he will not be killed. Under unique situations, such as separation from comrades, holding the advanced trenches, or when confronted by new enemy devices, fear possesses the soldier. When surrounded by companions it moves him to find protection in the group consciousness, to submerge his personality, to automatically obey and even to welcome severe discipline. His fear for personal safety is repressed; to express it while in the danger zone would incur the penalty he desires to avoid. So men fear personally yet never communicate it until out of danger. We need therefore to distinguish between the periods of intense emotion and accustomed situations on the one hand and the period of self-consciousness and introspective analysis. Soldiers who have passed fearlessly through an enemy barrage and succeeded in attaining their objective, when suddenly aware they are isolated or alone invariably experience fear, though it may be momentary. Most accounts agree that the fear of death for the soldier is not as terrifying as it is for the civilian. Their intimate acquaintance with it destroys much of its terror but the soldier actually struggling with an enemy and the self-conscious soldier are two different beings. The opinions of a man behind the lines and one just fresh from the conflict often vary and the same soldier finds it difficult to analyze his feelings without contradiction upon the same occasion.

Perhaps a few quotations may help to clarify a difficult problem. In the early stages of the war an officer wrote:

"Pain must be a relative thing, or some people wouldn't stand it better than others . . . . It can't be the colour of blood that's so upsetting; . . . . it must be because it's wet. Pity we can't bleed something dry . . . . Of course to be at war and not get accustomed to the sight of blood is an impossibility. You get callous of it when you see men laughing though they're covered with it, fighting though they can't see for it, dying for the loss of it."

"And just as you get to think of blood as a customary sight, so you become accustomed to the idea of death. It's all around you, not remote as in times of peace. It's peace that makes the love of life so falsely precious. In places where a man carries his life in his hands,

and in war, he sees death in its right perspective, which means, oddly enough, that it's shorn of its terrors. Perhaps it needed a great war like this to bring things into focus again." (71, pp. 15-16.)

From this we might conclude fear was entirely lacking, but why is blood "upsetting?" Why the pity of its very nature? The same man in a later letter commenting upon the bravery of his men remarks:

"Bravery, when you come to think of it, is one of the most fluid qualities. There is such a thin line dividing it from cowardice, and the name of that line, which sometimes is stretched so tightly that it has to snap, is not, remark you, lack of courage, but of nerves. If a man's nerves are good he stands a good chance of doing something decent; if they're not, all the more honor to him if he stands at all and does not run away." (71, p. 130.)

In this brief discussion, the factors inherited and acquired which increase nervous resistance to adverse and destructive stimuli cannot be enumerated, but is it not evident that bravery and courage are not evoked by compatible emotions but are the end results of fears, contravalent results which are uncertain, bordering upon flight or resistance? President Hall in his study of fear records:

"Yet fear has its fascinations, and strong, adventurous souls not only face danger when it comes, but go forth to meet it. Cowardice thus has its countervalent impulse in courage. The prospect of pain acts as a tonic and one does not need to be a hero to love to take risks and to venture in order to have. Not only is one measure of values the dangers we will face to attain them, but curiosity, lust for knowledge, wealth, power, ambition, control if they do not cast out fear. The great culture heroes set men free from fears. Without known danger life would be tame, insipid, asthenic. Men fight best if rightly afraid and even weak animals which would fly when brought to bay fight with the energy of desperation." (26, p. 153.)

To anticipate a study of behavior during the charge, several accounts may be used here with value. Reflecting upon the moment of advancing one says:

"There on the open field of death my life was out of my keeping, but the sensation of fear never entered my being. There was so much simplicity and so little effort in what I had done, in doing what eight hundred comrades had done, that I felt I could carry the work before me with as much credit as my code of self respect required." (45, p. 72.)

Of the same experience when engaged in ambulance work he continues:

"The harrowing sight was repellent, antagonistic to my mind. The tortured things lying at my feet were symbols of insecurity, ominous reminders of danger from which no discretion could save a man. My soul was barren of pity; fear went down into the innermost parts of me, fear for myself. The dead and dying lay all around me; I felt a vague obligation to the latter; they must be carried out." (45, p. 77.)

Of an occasion during a retreat in the same battle which lasted for days he says:

"I had got beyond that mean where the soul of a man swings like a pendulum from fear to indifference, and from indifference to fear. In danger I am never indifferent, but I find that I can readily adapt myself to the moods and tempers of my environment. But all men have some restraining influence to help them in hours of trial, some principle or some illusion. Duty, patriotism, vanity and dreams come to the help of the men in the trenches, all illusions probably, ephemeral and fleeting, but for a man who is as ephemeral and fleeting as his illusions are, he can lay his back against them and defy death and the terrors of the world. But let him for a moment stand naked and look at the staring reality of the terrors that engirt him and he becomes a raving lunatic." (45, p. 161)

Another soldier says that when leaving the trenches fear is greatest.

"The fear of being hit by shell or bullet was a hundred-fold greater than it had been during their part in action, when the risk was easily a hundred times greater, and more sympathy was expended over one man 'casualtied' coming out than over a score of those killed in the actual fight." (5, p. 123)

Kreisler who spent four weeks in the Austrian trenches believes that a man becomes indifferent to danger if the organism is worn down and the brain and faculty of perception is numbed by physical exertion.

Norman Hall describing the attitude of a bombing party writes:

"They went to their places with that spirit of stolid cheeriness which is the wonder and admiration of everyone who knows Tommy Atkins intimately. Formerly, when I saw him in this mood, I would think 'He doesn't realize. Men don't go out to meet death like this.' But long association with him had convinced me of the error of this opinion. These men knew death or terrible injury was in store for many of them; yet they were talking in excited and gleeful undertones, as they might have passed through the gates at a football match." (27, p. 180)

If any general conclusion may be drawn from these few quotations, and the war literature contains many references to fear and death, we may say that soldiers experience fear, in degrees determined by individual differences, the situations in which they are placed and the intensity of the emotive state. Towards the death of comrades they are indifferent, due probably to the inner conviction that they themselves will not be killed, while to the customary dangers of normal trench warfare they become almost entirely indifferent. If fear is ambivalent in character, we would therefore expect the soldiers in the trenches to be comparatively cheery and hopeful and this is exactly what we find. Perhaps this surface gaiety, cheerfulness and indifference, has given rise to the belief

that the soldier does not fear. May it not be more plausible that these forms of behavior, dominated by the unconscious, are securities against fear, countervalent expressions whose source is the most persistent and primitive of all the emotions?

During the early days of the war, opposing armies prevented from fraternizing resorted to banter and to a certain degree within each army the same kind of raillery prevailed. President Hall has said that "Psychic jest and joy are greatest when expressed to overcome fear. To substitute a joy for dread constitutes a distinct element in laughter." (Lecture notes, 1916.) The humor of the trenches is naïve, often mere nonsense, but it banishes fear and puts despair to flight. In addition to the standard jokes are the soldier songs, mere parodies of national and patriotic airs or else typical vaudeville "successes" full of references to the female sex. Although the evidence is meagre there is sufficient to postulate that sexual subjects afford much of the material for conversation, jokes and songs. Environed with the horrors and filth of the trenches man loses the veneer of civilization, becomes bestial and unreservedly vents his strong sexual passions. In contrast to the vulgar, repulsive and regressive behavior, necessary perhaps for uncultured men to deaden the mind to more destructive forces, there are those who combat filthy jesting, who yearn to possess again individual freedom, who depend upon the higher sources of a vivid imagination, a dramatic sense, to drive away the fear element and to curb reversionary influences. A representative of this class writes:

"Probably there is no one to whom this saving grace is more essential than to the fighting soldier, especially in winter. Every detail of his life is sordid and uncomfortable. His feet are always damp and cold. He is plastered with mud from head to foot. His clothes cling to him like a wet blanket. He is filthy and cannot get clean. His food is beastly. He has no prospect of anything that a civilian would call decent comfort unless he gets ill or wounded. There is no one to sympathize with his plight or call him a hero. If he has no sense of the dramatic, if his horizon is bounded by the sheer material discomfort and filth which surround him, he will sink to the level of the beast, lose his discipline and self respect, and spend his days and nights in making himself and every one else as miserable as possible by his incessant grumbling and ill-humor." (28, pp. 194-5)

Apparently the number of men who rise above their environment, who master their passions, and retain a grip on personal acquirements is small, but all soldiers unanimously acclaim the humorous sense as a saving grace; it aids to dispel their fears and it creates a necessary atmosphere of cheerfulness.

In the trenches the men in a large degree feel detached

from the world. Some writers have declared soldiers seldom think of home and friends. If this is true it is because they do not have sufficient opportunity and we must remember the field of consciousness is limited. Carrington reports:

"Letters from home, and journals, as they arrive, afford some slight, mental stir and commotion, for a time; but even these seem to leave no durable trace upon the mind, and their images and memories are soon obliterated. Thus a young corporal, in trying to analyze his impressions at the time, said, 'I am not sure that I thought of my family particularly, even when writing home! There seemed somehow to be a veil between us, shutting off all communion of feeling and interest between us.'" (9, p. 60)

If we can accept the personal character of the numerous letters as a criterion of the soldier's detachment from family and friends, there seems to be meagre evidence to prove that he is not conscious of them when writing. True, the men live practically in the senses, think only in a circumscribed manner, converse chiefly of simple things, resort often to gestures, and are dominated by the physical pleasures and pains but they do not entirely forget the past, certainly not their homes. N. S. Hall said that not once, during fifteen months of British army life did he hear a discussion of mothers, but he accounts for this by saying the soldiers realize the "futility, the emptyness of words in the face of unspeakable experiences."

The war has changed men's religious conceptions. R. J. Campbell states that there are not less than sixty thousand priests serving with the belligerents on all fronts which number does not include the priests of the Russian Eastern Church, or the protestant ministers serving with the British. (6, p. 13.)

During the early days of the war there were many accounts of supernatural interventions of which the best known refers to "The Angels of Mons" but these probably had their origin in the civil zone. Machen, I believe, justly claims credit for the Mons story (47). Religion in the trenches is shorn of its formality and ritual. The Roman Catholic adherents persist in observing their forms but the soldier's personal relation to a higher power has undergone a tremendous change. Not all, but the majority of the men become fatalists. Religion has been associated with observances and self-righteousness or with not drinking, not swearing, possibly not smoking and the avoiding of doubtful characters, and as trench life rather fosters most of these evils they do not want religion. Then their whole experience seems to negate the ideas they have of God and goodness. Furthermore the religious man

often worries, he weighs motives, is introspective and at times is doubtful if he is fit to die. Religion is to be avoided therefore because of this worry and its deterrent power, also because it stands as a sentinel against their immorality. Although they continually display many virtues such as unselfishness, sacrifice of personal safety and kindness, these are never connected with Christianity. There are exceptions. There are men inspired by lofty religious ideals, who feel impelled to battle or to serve in some way because of its righteousness. We can however justly affirm that fatalism prevails as the dominant characteristic in this respect.

One of the strange phenomena of the war is the great lack of hatred of the enemy, at least this is true of the British. Under peculiar circumstances such as when enraged by unnecessary violence or brutality, temporary states of intense hate may prevail but the trench warfare has been characterized by its absence. The soldiers do not revel in killing; they seldom think of the enemy in person; their warfare is mechanical. The prime motive for killing is self-preservation. They kill the enemy that they may not be killed. Such phrases as "I kill Fritzie because if I don't he will kill me," and "It was his life or mine" are very commonly expressed. Hankey perhaps has best defined the soldier's attitude:

"The Cockney warrior does not hate the Hun. Often and often you will hear him tell his mate that 'the Bosches is just like us, they wants to get 'ome as much as we do; but they can't 'elp theirselves.' At times he has regretful suspicions of the humanity of the Prussians and Bavarians; but they are not long-lived, and even while they endure he consoles himself with the proved good fellowship of the Saxon." (28, p. 91)

Even the sniper, who notches the butt of his rifle every time he scores a hit, will tell you he is is proud of his marksmanship, but he instinctively feels that every man he kills saves the life of a comrade. The artillerymen regard the enemy still more impersonally. They never see him in battle, yet they are subjected to intense fire from his guns. Their own success is reported by telephone, their fire is directed by observers and their attention, in an attack, is focussed upon comrades. It is only after the battle is over that they learn of their ultimate success or failure.

The effect of artillery fire is psychical as much as physical. The men behind the guns and those in the trenches have a greater nervous resistance when equal or superior to the enemy in shell power. Kreisler aptly puts it:

"The moral effect of the thundering of one's own artillery is most extraordinary, and many of us thought that we had never heard any

more welcome sound than the deep roaring and crashing that started in at our rear. It quickly helped to disperse the nervousness caused by the first entering into battle and to restore self-control and confidence." (37, p. 22)

Previous to the period of the attack it appears then that the soldier is motivated by the instinct of self-preservation and that he thinks little of the enemy in an abstract way.

Just as there are individual differences manifested in other phases of war, so before the period of attack and during the attack itself these persist. There are however more psychic and physical experiences shared by all during these occasions so that the dissimilarities are almost negligible. While waiting for the artillery to prepare the way, the men are in a state of tension, of extreme excitement and intense emotion. The heart beats faster, the hands tremble, the knees shake, the whole body is flushed with blood and it perspires freely. Crile has presented a valuable physiological interpretation of these phenomena. He says:

"His brain (the soldier's) is activated by the approach of the enemy. The activated brain in turn stimulates the adrenals, the thyroid, the liver. In consequence thyreoiodin, adrenalin and glycogen are thrown into the blood in more than normal quantities. These activating substances are for the purpose of facilitating attack or escape. As the secretions thus mobilized are utilized in neither attack nor escape, heat and the muscular actions of shaking and trembling are produced. The rapid transformation of energy causes a correspondingly rapid production of acid by-products. These increased acid by-products stimulate the respiratory center to greater activity to eliminate the carbonic acid gas. The increased adrenalin output mobilizes the circulation in the limbs; withdraws blood from the abdominal area; causes increased heart action and dilatation of the pupils. In addition, the increased acidity causes increased sweating, increased thirst, and increased urinary output, all of these water phenomena being adaptations for the neutralization of acidity." (13, pp. 19-20)

The few retrospections we have of this period invariably emphasize these bodily phenomena. They are the effects of a deep-seated cause. This is not solely the noise of cannon or the other concomitants of accustomed trench warfare for these are daily experiences. For months the trench has afforded shelter and protection, but soon the soldiers must go over the top, face in the open the hail of shrapnel, machine gun and rifle fire and if these are survived there must certainly follow the bayonet encounter. Experienced soldiers are not affected so much as younger men. The former say they do not fear but the latter are fearful. If we may judge their fears by their behavior the evidence is in favor of fear and to a lesser degree of anger being the causes of the physical and mental states. Fear is not present as a decision, "I fear

the future," or "I know death awaits for me," it may only rarely enter the focus of consciousness, but as an unconscious factor, a primitive and racial protection, it dominates the whole organism in the reflective hours before the charge. In this period the mind is clear despite the emotional stress. Thought is rapid, not abstract, but usually concrete imagery. Images of home, parents and friends, and memories of the past are vivid and pleasant, and even trivial incidents flash up. It is a common occurrence to exchange addresses and to pass on requests all relating to relatives; instructions in case of death. In a dim way most of them feel they will survive; they never reason about death yet they do not forget it. Some become reckless, and need to be restrained from jumping over the parapet, others are sustained and inspired by lofty feelings. As the crucial time approaches the memorial impressions fade and the attention is directed upon the enemy and the means of protection. One man said "Fear is before the blood is hot, or when told of the attack" and it is reasonable to believe that fear of danger, of open places, of unseen and uncontrollable forces, and for some perhaps of death itself, initiates the varied behaviors, physical and psychic, in this stage. As a result of the glandular secretions and the corresponding mental strain muscular activity becomes necessary, naturally therefore the first feeling experienced when over the parapet is one of relief. One soldier relates that upon the command "Fix bayonets!" he had a sinking sensation, a feeling of collapse, and his hands trembled violently but a sudden change occurred when commanded to charge. (35a, p. 39.)

The charge marks the culmination of the process of psychic degeneration; there is a complete abeyance of the mental processes, a final narrowing of the psyche. All images and other impressions extraneous to the great directing instinct of self-preservation entirely vanish. The mind is focussed upon one end, to protect the self. To get to the enemy as soon as possible is now the great desire but there is no haste in the forward move. Here lies a difference between the French and British. The former rush impetuously and intrepidly, the latter follow with precision the steadily advancing barrage. Some regiments, especially the Scottish, brandish their arms, shout and utter loud cries, while others are dogged, silent yet irresistibly determined. A peculiar elation takes possession of the mind comparable to a feeling of buoyancy or exhilaration. Every vestige of fear vanishes; it has passed over into anger and the positive bodily actions necessary to

preserve his life. Man is now a brute-beast, shorn of his higher instincts and culture. He is aware only of the group which he unconsciously feels affords protection. He takes no notice of his fallen dead comrades, at times he may use their bodies for cover, while even the cries of the wounded do not deter him from pressing forward. Stretcher bearers who advance with the infantry tell us the faces of the attackers bear savage expressions similar to preying animals. Like animals, only one idea, viz., self-preservation, dominates consciousness, and this is best assured by killing as many of the enemy as possible.

When he meets the enemy his purpose is complete destruction. He does not think of him, there is no pity, no sentiment, no thought of chivalry. Some mysterious force seems to impel him forward. During the earlier stages of the war there was no animosity, no hatred; the man he killed was not a person, a German, but simply an enemy. Recently this has changed because of the outrages which the men have witnessed so that anger and intense hatred are powerful activators. The following quotation characterizes what was the attitude of most soldiers:

"I asked Zeni Peshkoff, socialist, what his sensations were when he went out to kill. 'It didn't seem real, it doesn't now. Before my last charge the lieutenant and I were filled with the beauty of the night. We sat gazing at the stars. Then the command came and we rushed forward. It did not seem possible I was killing human beings.'" (M. Z. Doty. Short rations, pp. 61, 62)

The first contact of the soft body of the enemy upon the bayonet and the gush of the hot blood produces a sensation of horror which almost paralyses the victor but this soon passes off as he realizes the more he kills the less chance there is of being killed himself, in fact this horror finally becomes a thrill of pleasure. To cite from Carrington's article; a man was asked whether he felt horrified when he bayoneted his man,

"Not at all," he replied; "I had a curious sensation in my arms as I felt the soft body, and I grew fatigued with continued fighting. But the action was of such short duration, and I felt all the time so keenly that I was fighting for my life, and seeking only to preserve myself, by killing the enemy that I gave no thought to him." (9, p. 66)

From this he concludes "The act of killing does not shock; that is established beyond a doubt." Such a conclusion overstates the actual effects for the impression made upon the nervous system, and conserved in the unconscious, at some future date may possess the soldier's mind, causing severe mental disorders. Just as the soldier thinks only of the enemy,

so he ceases during action to be governed by the ethical code of the civilian. He does not question the morality of his acts, he never thinks of it. In a similar manner he is unaware of the bravery of his deeds. Courage is now determined by circumstances and the emotional intensity; it is not the result of reason and does not necessarily depend upon physical prowess and strength.

The attack emphasizes the importance of the officers and of the long period of trench discipline. As a result of the intensive training, in actual combat it is easier for a single principle to sway the whole being; for the mind to become non-rational and simple. The officers must now direct the proceedings entirely, they must determine the extent of the attack for the men are intent on their own protection, the killing of the enemy, and in this they require both encouragement and restraint. Furthermore the leaders determine the morale of the troops who instinctively are imitators and who regard their officers as symbols of duty, discipline and the nation. At times the loss of an officer may terrorize a company and cause disaster to a regiment.

Other occasions of conscious fears are few during combat perhaps the typical one being that of isolation from the group, when fear results in flight or other means of escape.

While the charge represents a regression in phylogeny and initiates a period of control by instinctive reactions where the emotions and behavior function for self-preservation, it does not parallel the stage of the love of killing nor even animal levels entirely. Only in rare cases will a soldier kill the enemy when he signals his surrender which goes to prove that the theory of struggle in a causal sense is ever subject to the real, deeper motive of protection.

The intensity of the fighting produces great physical changes. Usually the soldiers leave the battle fatigued, showing signs of the strain on their faces and limpid bodies while often they are hysterically cheerful, probably due to the nervous reaction. The ordeal upon reflection seems a long one, as one man in answer to the question "What sort of a week have you had;" put it, "It hasn't been a week, son, it's been a lifetime!"

In modern trench warfare armies seldom suffer a retreat, but usually a series of repulses. The attackers and defenders experience similar psychic changes except that the latter if subjected to intense bombardment suffer from nervous collapse due probably more to physical katabolism than to psychoses. If reverses are common there follows a lowering

of the morale. A retreat may be of the nature of a panic, when fear, rising above the unconscious, gains the final path, or it may, as it often is, be a methodical falling back, marked by continued fighting. Artillery support has changed the nature of fighting so much and the contestants are so evenly equipped that to put an intrenched army to flight is almost an impossibility.

In the great retreat through Belgium and Northern France the factor of fatigue was important as a determinant of conduct. At first the men were surprised, then discontented and resentful, and later owing to the loss of sleep and a resulting fatigue they grew apathetic, indifferent to the finer sensibilities, to sorrow, hope or happiness, until anger, rage and discontent supplanted their opposite emotions. The mind is concentrated upon the body which is oversensitive to pain. It appears that the visual and auditory thresholds are raised for they no longer function efficiently. Just as in the charge so now behavior is a reaction to the protective instincts, but whereas in the former the glands function excessively, thereby increasing nervous and physical resistance, in the retreat such secretions are prevented by the toxic products, due to fatigue. Finally the desire for sleep is more powerful than the will to live; the mental yields to physical demands. So men slept when marching; when in position for firing sleep overcame them; they slept when death was near; some were captured asleep. This great continued retreat, lasting nine days, during which men marched one hundred and eighty miles burdened with their equipment, fighting losing battles, and at all times held in check by promises of ultimate success and needed rest, illustrates the tremendous sustaining power of the psychic and nervous mechanisms.

When the moment of the advance came after a brief period of rest the weakest men were brave, all experienced feelings of elation, they were lifted above their apathy, and the whole physical organism tingled with excitement. Of the many factors producing such a transformation the important are: the opportunity to meet the foe upon equal terms; the release from continued purely defensive effort, or the thrill of the offensive, of superiority; the powerful motive of revenge; and the desire to excel in heroic actions, which is an imaginative reverie.

Soldiers wounded in the charge or retreat are not aware of the degree of their injury until unable to use the affected member. Thus a rifle bullet causes a burning sensation which passes unheeded when not received in a vital organ, and some men who had a limb shot away said they did not notice it

until they fell on the shattered stump. If able, wounded men instinctively seek cover either near at hand or they crawl to the trenches. When conscious the rational processes often return, the men think of more severely wounded comrades; they are chivalrous, heroic, brave, they will seldom monopolize ambulance facilities until these are removed. This is not the bravery of unreflective moments, it does not parallel the heroism of the charge, for the men realize their danger, and they are torn by fears. It is due to the reassertion of the higher mental processes, to the establishment of the normal relation of the conscious and unconscious which again function the sympathetic emotions. If cognizant of immediate death their thoughts are of home, and sometimes of God and the future, but just as often they ask about the result of the battle and the welfare of comrades.

No generalization can be made about the psychic states of the wounded as these are determined by the seriousness of the injury, more especially the organs affected. In the same ward are men whose language is obscene, who curse God and country, who rave about sex and immoral conduct, who rail their neighbor because his gangrenous wound emits nauseating odors and whose only thought is of a world turned against them; while others are courteous, childish in simplicity, mindful of comrades and nurses, who bear pain stoically, who seek comfort in religion and who often think of home and family. If they know they cannot return to the fighting line their attention is focussed upon the immediate present, the future is too uncertain, too terrible to contemplate; these painful thoughts apparently are suppressed unconsciously. Few in number are those who anticipate with pleasure the return to combat, the majority abhor war, their experiences are too unreal, too horrible, and usually their reply to questions of the future are, "It's the business of war," or "*C'est la guerre, que voulez-vous.*" When asleep their dreams are about the enemy, the battle and their part in it; they cry out, pull their bandages off and often endeavor to leap out of bed even though severely wounded, while many others are disturbed with sexual phantasies, which likewise are communicated by word and action. We cannot therefore generalize or point to a typical psychic state as in the charge but we may believe that to these men war is repulsive, not to be discussed, and that the present is all-important.

To what extent the experiences of the great war affect the soldier-citizen in his re-established civic life is for the future to disclose.

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349

VISUAL, CUTANEOUS, AND KINAESTHETIC GHOSTS

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A. Introduction

The questionnaire method, which has been so often employed in this occult field of psychology, has failed to yield sufficiently consistent results to enable one to formulate a satisfactory theory of visual ghosts. Unfortunately, ghost stories, like fish stories, are too often either pure creations or gross misrepresentations. People can always be found who deliberately distort facts as long as they realize that no one is qualified to contradict their statements. False ghost stories are often motivated by beliefs in the supernatural, or by the mere desire to interest or scare others. A ghost story of one who has been trained from his cradle days to be an aggressive spiritualist, is generally of a more exciting nature than one told by an anti-spiritualist. Ghosts and rumors of ghosts have ruled societies throughout the ages of human history; and the reign of the rumors has perhaps caused the greater terror. Those investigators, who have made use of the questionnaire method, failed utterly to distinguish between ghost and mere rumor. We shall censor these stories and shall limit ourselves to only those ghosts such as have been produced and reproduced under controlled conditions.

It is a duty of the psychologist to attempt to account for the fact that a *limited number* of people, apparently of all races, have observed certain appearances which they have been willing to call ghosts or spirits. The visual ghosts will be of primary interest to us; but, in order to make our theory more comprehensible, we shall discuss also a number of other phenomena which are fundamentally the same as the visual ghosts, and which are fortunately not shrouded in super-

stition. The latter phenomena we may profitably call cutaneous and kinaesthetic ghosts.

B. Visual Ghosts

1. *The Nocturnal Visual Ghost*

Experimental results indicate that this traditional ghost is only a *positive after-image of long duration*[1] which becomes revived by a stimulus of such extreme insignificance that the *immediate* visual effect produced by it may find no place in the ghost story. If we choose to speak in Pawlow's terminology, we may call this ghost a *conditioned* visual response to a previously perceived more or less complex pattern of visual stimuli.

In a previous article[2] I related one of my experiences with the positive after-image which I suggested might be of interest to spiritualistically inclined persons. This was an account of my illuminating my retinas through my closed eyelids and thereby reviving a positive after-image which I had experienced about fifty minutes before. Recently I have succeeded in reviving remarkably distinct positive after-images from six to eight hours after the original ones were experienced. I waited in the darkness from five to ten minutes, and then fixated and illuminated my hand or a particular white surface steadily for about one second. I observed a continuous positive after-image which often lasted as long as a minute and a half. Then, after sleeping six or eight hours in a very dark room, and before opening the eyes, I illuminated the retinas with a flashlight, but so very slightly that I did not become aware of the red of my closed eyelids. Not every morning, but sometimes, the after-image was revived. It occasionally appeared in almost its original distinctness, and once or twice it endured about as long as originally. The ghost frequently appeared the first time the retinas were slightly illuminated, but sometimes it did not. When the first trial proved unsuccessful, I illuminated the retinas again and again until it did appear, or, until I gave up in despair. I have succeeded in reviving a single positive after-image a number of times, but not in immediate suc-

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[1] A positive after-image of long duration and the essential conditions for obtaining it are described by Helmholtz in his Physiologische Optik p. 504. On p. 506 he states that the positive after-image of bright clouds viewed thru a window for about ⅓ second, disappears after about 12 seconds.

[2] Positive After-Images of Long Duration, this Journal, XXVII, p. 334.

cession. I made each time a pause of just about as many seconds as the ghost had remained with me, and then I slightly illuminated the retinas. When this rule was strictly obeyed, I found it possible to revive the after-image five or six times in this slow tempo. It seems that, in order to revive an after-image after such a long lapse of time, the retinas must be illuminated just as the otherwise unnoticeable positive after-effect is on the verge of reappearing. That a positive after-effect does reappear a number of times without the retinas becoming reilluminated, is a matter of frequent observation. It is observed to grow fainter and fainter until it can be no longer detected; and we shall assume that at least some of the visual structures involved, continue thus periodically to refunction for an indefinite time. This, we shall suppose, is especially the case when the individual is inclosed in darkness, under which condition the retinas are not being strongly stimulated.

If my retinas should have become similarly illuminated in the morning by something which was not under my control, as was the flashlight, I should very likely have seen a ghost when I was not looking for it. This would more likely occur if I should turn out the light, and, after remaining in the darkness for a few minutes, turn on the light for a short interval, perhaps to see if a certain article is in its proper place. If my retinas should happen to become properly illuminated a few hours later, I should very likely see a ghost of the last observed object. If such should appear during my sleep, I should be inclined to call it an unusually vivid dream, the nucleus of which is this object; I might just as well consider it a revived positive after-image or a ghost.

Let us now imagine the following possible conditions. Two persons, A and B, are in a room and are ready to go out. A turns out the light, and they depart in the darkness together. After a few minutes, B discovers that he has forgotten his watch, and they both return. A turns on the light long enough for B to get his watch and looks at him during the short interval. They then go away for good, separate, and A's path leads him through a dark forest. While in the forest, he chances to pass through a small glade and thereby slightly illuminate the retinas just as the ordinarily unnoticeable positive after-effect of B is on the verge of reappearing. In the darkness beyond the glade, A will then see a ghost of B, but will he recognize it as B's image? These ghosts are not always so distinct and clearly defined that one can say with certainty that it is the image of this and not

of that person. If A should be spiritualistically inclined, we might expect him to say, "Oh, that's the spirit of my dead friend." He may, in telling his ghost story, relate how the spirit or ghost of his dead friend approached him one night in a dark forest; how it floated about him for a time; and how it then either gradually or suddenly vanished; and not even think of mentioning the fact that he had just walked through a glade.

There are many other conditions under which the ghost might appear. On a dark cloudy night, A might be walking in the open when the moon chances to shine through a small break in the clouds just at the right moment to revive the after-image. A might also glance at a distant light, or even at a near-by bright surface which stands out in the darkness, such as a white tomb-stone he sees while passing a graveyard. Or, if he should be brave enough to approach a deserted house or castle, where, as he has been told, horrible crimes have been committed, he might be expected to glance rather carefully at all the bright objects near him; and furthermore, what is equally important, he might be expected to walk rather timidly and occasionally stand perfectly still. This latter condition is important, because a positive after-image of long duration does not appear, or, if it has already appeared, it either grows weaker or vanishes completely when one makes a pronounced sudden movement. Experimental results indicate that A might easily keep the ghost away by whistling. He might also put the muscles of his body under tension, perhaps slap his hands together, or shake his fists in the air, and make the further consistent response of saying, "I am not afraid," and thus reach the house or castle without being intercepted by a ghost.

The two following ghost stories are typical of a large number of the many hundreds which have been related to me since I first began serious investigations in this field. A certain French duke, while sleeping in his château, was awakened by a peculiar knocking at his door. Almost immediately, a human figure with two heads entered through the closed door, floated about in the room for a short time, and then gradually disappeared through a wall. This gentleman related his experience to an elderly lady who was at the same château. In an excited way, but with an honest look, she said, "Just about two months later, I had *exactly* the same experience. I was awakened by *exactly* the same knocking at the door, and, thinking it was a maid, I said, 'Come in,' but no one responded, and I thought immediately of the two-

headed person. Well, as you may imagine, my heart almost stopped. I was absolutely *powerless,* and I did not dare look around, because I knew that two-headed person was there behind me." When I asked her if she saw anything, she replied, "No, but everything told me it was there *just* behind me." If this lady had been untruthful enough to emphasize her ghost story by asserting that she really saw the ghost, I should have considered this a typical case of contagious ghost seeing. Or, if she had glanced at a bright object before she became 'powerless,' as the duke perhaps did when he heard the noise, she might have thereby really caused a positive after-image of some previously perceived visual pattern to be revived. This would have been a case of real ghost contagion, due to the fact, however, that the duke's story caused her to behave in an ideal way to cause an after-image to be revived. The duke's story caused her to become, as she expressed it, 'powerless'; and we might here remark that a fixating person is a 'powerless' one. It might also be mentioned in this particular connection, that a fatigued person is likewise, often at least, a fixating person. Observations seem to indicate that the well known hypnagogic phenomena, which appearances are fundamentally the same as our ghosts, generally arise under conditions of unusual muscular fatigue. That the duke's ghost possessed two heads, is nothing unusual in a positive after-image. One can easily obtain a similar effect by fixating two persons, one of which is somewhat behind the other, by turning the head to the right or to the left while fixating a single person, or by holding the head and the eyes still while the fixated person moves only the head.

There is really no apparent reason why many of the traditional ghosts should not be the revived positive after-images of deceased persons. In this connection it is interesting, that while observing corpses, people are not ordinarily engaged in making such pronounced movements as are involved in the activities of dancing, laughing, whistling, or perhaps singing such popular songs as are accompanied with pronounced bodily movements. On the contrary, even those who do not actually weep often stand before the corpse and fixate it carefully, sometimes so very carefully that many of the objects in the field of vision subjectively disappear. Others, in weeping, shut out for a long time the light rays from the eyes, take another glance at the corpse, and then weep again in the same way. The weeping is often, at least after several hours of frequent weeping, of a rather silent nature, in the

sense that no very pronounced movements are involved in it. These are ideal conditions for preparing one to see ghosts. It is by no means necessary that the mourner should at the time see what we call a positive after-image of the corpse; for a large part of the percept is, after the first few moments of exposure, no longer dependent upon the objective stimulus for its continued existence, and is therefore fundamentally the same as the positive after-image which, under the condition of darkness, exists independently of the objective visual pattern. The positive after-image is evidently in progress while the surface is being observed. It is only the positive after-image, and not the entire percept, which can be revived by an insignificant visual stimulus; the entire composite visual experience can be revived only when the original objective pattern is presented under exactly the same conditions as previously.

If the mourner should leave the corpse and pass immediately into the darkness, the likelihood of his later seeing a ghost would be greater than if he should leave and remain for a while in daylight, where the retinas become in the meantime strongly stimulated in a variety of ways. In this connection, I must recall the fact, that after I had spent about forty minutes in a well-lighted room, and then about ten in the darkness, a ghost of a previously fixated person appeared when I illuminated the retinas through the closed eyelids. Whether this was the ghost of the person as I last saw him in the light, or whether it was the revived after-image of him which I had observed about fifty minutes before in the dark-room, I now feel unable to say, for, as I distinctly remember, he stood before me in about the same way in both instances. However, a few persons insist they have seen ghosts of me even many days after I showed them a positive after-image of myself. It seems as if the Chinese see such ghosts more frequently than people of other races. This, I think, should be considered in connection with some of the facts I reported in my article on 'The Biological Significance of Eye Appendages of Organisms'[3]; a study of this sort might possibly throw considerable light on many of the traditions of the Chinaman.

We have previously assumed (p. 2) that at least some of the structures involved in producing an after-image pattern periodically refunction for an indefinite time; and, that if when these few structures are just on the verge of refunc-

[3] This article has been accepted for publication in this journal, but has not yet appeared.

tioning, the retinas chance to be restimulated in somewhat the same way as originally, these, and also many other visual structures which previously functioned simultaneously with these, will refunction and accordingly produce a distinct positive pattern. This postulation was made in order to explain the fact that a positive after-image can be revived only *occasionally* by a given insignificant stimulus. Certain facts seem to support our assumption. It is not a theory, but a matter of observation, that a positive after-image often alternates with a negative after-effect of about the same duration. When the positive effect lasts thirty seconds (this seems to approximate the maximum duration for the normal untrained individual who fixates carefully), the duration of the negative one is also just about thirty seconds. After only two or three alternations, however, the negative effect of the white surface can be, as a rule, no longer noticed, while the positive one may be detected still several times. As I pointed out in the article on positive after-images already referred to, (p. 331 of that article), if the retinas become again illuminated in the same way just as the faint positive after-effect is on the verge of reappearing, the resulting after-image is unusually distinct. If the retinas become reilluminated with the previously used pattern just after the faint positive effect has vanished, a distinct positive after-image generally follows, but observations show that this one is almost invariably less distinct than the one which is caused to occur simultaneously with the faint one. On the other hand, this positive after-image is often of much longer duration than when it is caused to coincide temporarily with the faint one. Apparently, the duration of this effect amounts to the duration of the directly produced effect plus that of at least a part of the ensuing one that now becomes revived to an extraordinary degree of distinctness by the visual responses which are in progress when the otherwise faint after-effect begins to reappear.

Let us call this directly produced after-image A, and the periodically appearing positive after-effect which this time follows A, let us call B. Since both A and B consist of many visual responses, we may speak of the A- and B-responses, or of the A- and B-patterns of simultaneously occurring responses. A occurs, e. g., 15 seconds before B, but A is still in progress when B appears, and consequently, B is revived to unusual distinctness by the responses of the A-pattern. If this occurs frequently, the A- and the B-patterns become associated in the particular order AB, and later, when no positive effects of the objective pattern can be observed, great

numbers of the B-responses follow when only the A-pattern is produced directly. We should certainly expect that if positive visual responses can become associated to form a pattern of simultaneously occurring ones, they can also become associated in a serial order to form a positive pattern of longer duration. I am convinced, that through just such a procedure, I have trained myself to see positive after-images which now last for a minute and a half. Originally they lasted about one-third as long as now.

Another step which I took to increase the duration, and also the distinctness of the after-image, was to greatly diminish the eye movements which I found occurred almost invariably for a few seconds after the retinas were subjected to a sudden change in illumination. The eyes moved from side to side or up and down in various tempi, but usually in that of approximately 0.38 seconds, or about twenty-six times in ten seconds. I learned that, by frowning in a particular way, I could inhibit in a large measure all of these except the first movement. Only after much practice I became able to suppress this one to such an extent that it no longer caused a break in the positive after-image. When I do not suppress these movements, I am able to distinguish the cinematograph positive after-image and then a pulsating one of long duration, such as C. A. Young[4] described in 1872. The discontinuity of the positive after-image is more pronounced when the period of illumination is extremely short; but in any case, one can make the image continuous by carefully fixating, which process involves suppressing especially the eye movements. As Miles[5] has shown, it is possible to periodically illuminate the retinas in such a tempo that the usual pulsating positive after-image becomes not only a continuous effect, but also one of very long duration. Miles found it best to reilluminate the retinas at the rate of about four times per second. This tempo very closely approximates our tempo of eye movement which fluctuates, with different individuals, slightly above and below 0.38 seconds.[6] It would then seem

[4] Phil. Mag., XLVIII, p. 343.

[5] George H. Miles, The Formation of Projected Visual Images by Intermittent Retinal Stimulation, Brit. Jour. of Psychol., VII., 1915.

[6] Miles also observed certain involuntary movements of the eyelids (I think these were evidently due to the movements of the eyeballs) which, according to his rough estimate, occurred in the tempo of about 0.4 seconds. The effects of the eye movements can be observed unusually well if the retinas become strongly illuminated for a very brief time interval, and then a phosphorescent patch in darkness is observed. The objective stimulus appears to move in the horizontal or vertical direction in the tempo of about 0.38 seconds, thus producing alternately light and dark spaces where it at any time seems to lie.

that, in order to convert the pulsating positive after-image into a continuous effect, we can reilluminate the retinas at the beginning of each dark space.

II. The Diurnal Visual Ghost

The traditional ghosts which fall in this class are such as the revived positive *after-images* and *after-forms* of idols, sweethearts, corpses, or handwriting of particular persons; and they appear in daylight or by artificial light, usually in or on such backgrounds as smooth walls, open fire-places, clouds of whitish smoke, rainclouds, or deep walls. We are not concerned here so much with revived positive after-images as with revived positive *forms* of objective visual patterns. A positive after-image is the pattern of visual responses which is produced directly by the objective pattern fixated, and which may persist after the pattern is removed. The positive *form* which is of primary concern to us here, is the positive after-image plus the negative effect of the background of the fixated pattern; this negative effect coincides with the positive after-image, or in other words, it *assumes* the *form* of the pattern and this induced negative effect of the background and the positive after-image can later be revived simultaneously. Before discussing the *form* of the pattern, it would seem well to consider briefly a certain peculiarity in the behaviour of the mere positive after-image which can be revived in the daytime.

Hering[7] fixated a gas flame for about twenty seconds and then directed the eyes to a strongly illuminated white paper. He observed first a negative after-effect of the flame which soon passed over into a distinct positive one. He called attention to the fact that that portion of the white surface on which the positive after-image of a bright object develops, may be much brighter than the remainder of the white projection field. McDougall[8] fixated a white surface, waited until he could no longer detect any sort of an after-effect of it, and then revived the original positive after-image by turning the eyes to a white background. He also found that upon blinking, the positive after-image reappears and frequently remains for a few seconds after the lids are opened. Baird,[9]

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[7] Ewald Hering, Zur Lehre vom Lichsinne. Wien, 1878, s. 44.

[8] W. McDougall, Some New Observations in Support of Thomas Young's Theory of Light- and Color-Vision, (I), Mind, N. S., X, 1901, p. 55.

[9] J. W. Baird, The Color Sensitivity of the Peripheral Retina, Published by the Carnegie Institution of Washington, Wash., D. C., 1905, pp. 57-59.

also, calling attention to the fact that the effect of a visual stimulus may persist for an unusual time, and too that it may be entirely sub-liminal, made the following statements which should be of considerable interest to us: "A surprising fact in connection with these after-effects was the observer's utter ignorance of their existence. . . . We can only conclude that the functioning of the peripheral retina is followed by an after-effect which is tenaciously persistent and is wholly latent in character; and that this sub-liminal capacity is called into active functioning by subsequent stimulation." Troland's[10] researches lead him to make the following statements: "By use of dimming, after-images can be demonstrated for pre-exposures of an eighth of a second, which leave no noticeable trace on the undimmed field. . . . If the projection field is brightened, the faded negative after-image is *reversed,* and becomes positive. . . . The positive image fades on the brightened field, but upon dimming and rebrightening, again appears."

The revived after-images here mentioned may be anything, a positive one or any of the possible negative ones of a flight of colors which may follow after stimulation with a given color. As to what the particular qualitative nature of the first revived after-image is, depends in the first place upon which one of these possible ones happens to be in progress when the retinas are reilluminated, and in the second place upon the qualitative nature of the stimulus used for reilluminating the retinas. If the eyes are directed to an illuminated black surface, at least many minutes after a white object has been fixated, a negative after-effect can sometimes be observed; but, as Hering pointed out (however, in a special case) this soon gives way to a positive effect; and we might here remark that it is occasionally of an hallucinatory distinctness. These few special cases we shall call diurnal ghosts. The black background revives only indirectly the previously established pattern of white-responses. It produces directly great numbers of black-responses, some of which are associated with and call forth some of the white-responses, and among others, the associated cluster of them which is our previously established positive pattern. If the background is white, the positive white pattern may be produced directly, and the pattern of the previously fixated black background is then produced indirectly. We should never forget that we must reckon with

[10] L. T. Troland, The Influence of Changes of Illumination upon After-images, Paper read before the Amer. Psychol. Assoc. at New York, Dec., 1916.

the entire or general pattern and not alone with any small portion of it. When any portion of the general pattern is caused to recur, all the other portions may reoccur with it, and in the same temporal order as previously, which means simultaneity. Thus when the eyes are closed, a positive after-image of a white pattern may be revived indirectly, but immediately, because when the pattern of the black-responses is caused to recur, the pattern of white-responses occur with them to form a portion of the previously established general pattern.

Our argument concerning the existence of the positive pattern of a white object on a white background, naturally leads us to conclude that the distinctness of the visual effect is dependent upon the number of white-responses which occur in a unit of time when the given retinal areas are stimulated. When I direct the eyes to a white surface, my percept of that surface is characterized by a certain degree of whitishness, and if I make the surface objectively still whiter, the percept is now characterized by a still greater degree of whitishness; which indicates that the first stimulus did not call forth all the white-responses which I, as a visual organism, could manifest when the retinal areas in question were stimulated. The fact that the particular area of a white background upon which the previously established positive pattern of a white surface appears, is unusually whitish, would mean that the effect observed on this limited area is due to a large number of directly produced white-responses plus some others which could be indirectly produced by virtue of the fact that they were previously associated with some of those directly produced ones. Another way of discussing this matter is to say the objective stimulus produces, among many others, some of the responses of the positive pattern directly, and the others of the pattern occur simultaneously with these as previously.

But when we fixate a surface steadily for many seconds, we have still more to consider than the mere positive after-image of the fixated surface. This leads us to a consideration of the *after-form*. If I fixate a white area which lies on a large illuminated black background, the white surface will ultimately disappear. This occurs for two primary reasons. In the first place, the white-structures that function become highly exhausted and give way to black-responses which are associated in this particular temporal order with the white-responses;[11] and in the second place, the white of the white

[11] I do not see fit to take up space here to suggest why this association exists. I shall simply consider the fact that such an association is established.

area and the black of the black background previous to the exhaustion of the white- and black-structures, induce themselves over one another,[12] and finally there is no border line left between them. In the final after-effect, I notice that the two colors have changed places. We can better express what here takes place if we let W stand for the directly produced pattern of white-responses, Bk′ for the black-responses which are associated with and follow the white-responses of W, and Bk″ for the positive or self-induced effects of the neighboring black background. The distinctness of the negative after-effects of the white surface would then be dependent upon the distinctness of each of the two components Bk′ and Bk″ in the absence of W. The expression B′+Bk″—W indicates clearly that we must observe black when the white is removed. If we leave Bk″ out of account, as we can do in a degree by illuminating the surface for such a short time interval (from about ⅛ to 1 sec.) that the simultaneous self-induced effects are negligible, we find that the negative after-effect which follows the positive after-image of the white surface is very indistinct, and that it is in some cases not even noticeable. But Bk′+Bk″ form, in the absence of the objective white, a remarkably distinct after-form, and this form is all the more pronounced if the black background is, relative to the white area, quite large; *of two unequal areas of qualitative sameness the self-inductive effect of the larger one is the greater.* The distinctness of the negative after-form of the black background is determined by the distinctness of each of the two components, W′ and W″, in the absence of the objective black. Our shorthand expression which signifies the quality of this negative after-form would be W′+W″—Bk.

To further illustrate the fact that colors induce themselves over one another, we may form a double pattern of adjoining white and blue areas and carefully fixate a point on the border line between them. After this pattern has been fixated for only four or five seconds and the retinas are inclosed in darkness, we observe in place of the white a blackish blue and instead of the blue a whitish yellow. The self-induced blue can be seen even in the daylight if we fixate the pattern carefully for thirty or forty seconds. If the room is made

[12] The fact of the positive simultaneous induction has been recorded, first by Rollet, and then by W. McDougall, Some New Observations, etc. (III conclusion), Mind, N. S., X, 1901, pp. 348-353. See McDougall's reference to Rollet and the quotation given on p. 349 of the article just referred to.

dark while the induced blue is quite distinct, *this self-induced blue often remains positive for many seconds.* This would lead us to suppose that when the fixation continues for only four or five seconds, there is a sub-liminal blue induced over the white; that there is a sub-liminal white induced over the blue; and that these self-induced effects remain positive and merely become prominent when the objective stimuli are removed, as when all light is shut out from the retinas.

If a patch of paper which is qualitatively more similar to the blue than is the white is placed on the white area, this will become bluish more readily than will its white background. This patch does not need to be objectively bluish; it is quite sufficient if the similarity is one of brightness only, and the patch by no means needs to be of the same brightness as the blue; it can be a gray that is only somewhat blacker than the white. *The blue induces itself over both the white and the gray, but the more strongly over the gray which is the more similar qualitatively to the blue.* A variety of experiments show that *the greater the qualitative similarity between two areas, the more readily do the colors of these areas induce themselves upon one another;* and it is to be remarked that *this self-induction apparently does not heed retinal distances.* That these simultaneously self-induced colors seem to occur independently of retinal distances, would seem to indicate that the self-induction does not take place in the retinas, but at some place farther back in the visual organism.

If we carefully fixate from five to ten minutes such a pattern as any of those just mentioned, and then wait until no after-effect of it can be observed, the entire positive form of the pattern can often be revived with all the contrast effects, if we remain for a time in weak illumination and then fixate such a bright surface as a field of snow. The positive after-form of a white object soon disappears on the snow, giving way to a negative one; and the second positive after-form that appears, if it appears at all, is as a rule very indistinct. If now, after having fixated the snow, we decrease the illumination on the retinas by turning the eyes to a dark wall or by passing into a moderately well-illuminated room, everything in the visual field is often negative at first; but as the general negative effect fades, the positive form of the previously fixated pattern often comes into prominence. If the pattern fixated is made up of two adjoining areas of red and blue, this pattern can be revived by looking at the snow, and the red and blue may change places a number of

times before they completely vanish.[13] After they vanish, they can be again revived by directing the eyes for a short time to either a blue or a red surface, by closing the eyes so long that the red of the blood-saturated eyelids can be noticed, or by walking rather rapidly on the east side of and very near to a high picket fence through which the low western sun shines. The difference here between the positive and negative after-forms can be determined only by remembering the previous relative positions of the red and blue in the objective pattern; if the objective red lies to the right of the blue, and this same relative position of the subjective colors is present, the revived after-form is positive, otherwise it is negative. Thus the positiveness or negativeness of the general pattern is experienced only as the one or the other of the two possible positions of the colors.

One of my former students, in an attempt to observe what he had heard me speak of as 'the positive after-images of long duration,' fixated his mother for many minutes and then passed into a room which was only moderately dark. After many such attempts he succeeded in reviving a distinct positive after-form of his mother's face. He later complained to me that he had very much difficulty in securing the 'positive after-image.' He said it was quite distinct only a very few times, and that it was generally not to be recognized. When he told me of the odd method he used, it became at once evident that he had misunderstood the instructions, which had fortunately never been explained explicitly to him. I have followed out this new method of procedure, and a few times I have succeeded in reviving the positive after-form of the person's face by turning the eyes alternately to white and black backgrounds. Once I succeeded in seeing the white of one eye. The other eye had the appearance of a mere dark cavity in the head. As a general rule the eyes are represented by mere dark spaces, the nostrils seem to be very much inflated, the nose seems unusually short, the cheeks unusually hollow, and the ears are often two or three times their natural length, and quite pointed.

I recently discovered that if one fixates for five or six minutes, e. g., a moderately well-illuminated person, somewhat peripherally, and then steadily fixates a point on a large

[13] For the first description of this behaviour of the red and blue pattern on the snow, I am indebted to Mr. W. C. Bock, Research Fellow in Psychology at The Ohio State University. Mr. Bock said the red and blue once interchanged two or three times before they disappeared completely.

smooth wall for three or four minutes, perhaps in one trial out of fifty a ghost of the previously fixated person appears and stands out as if in high relief on the wall. When this ghost appears, it usually remains for only about four seconds, but after a lapse of about four seconds it reappears. I have observed it to appear thus perhaps as many as twenty or thirty times; fourteen such periodic appearances is the greatest number I have actually counted at a single sitting. Often the features of the person can be seen remarkably well, but sometimes the eye regions are too dark, the nostrils too large, and the ears too pointed to enable one to say whether it looks more like the person fixated or the Devil.

In this experiment, the visual structures for the color of the wall were first highly exhausted by steadily fixating the surface for three or four minutes so that the visual responses, which the wall at first called forth, would play an insignificant rôle in inhibiting the positive after-form of primary interest. The muscles of the iris became in the meantime well recuperated, began to contract and expand, and thus caused the peripheral regions of the retinas to become differently illuminated in the slow tempo of about four seconds. At each expansion of the pupils, the ghost appeared, and it remained present until the pupils contracted, thus darkening the retinas and accordingly reviving the negative form of the pattern. One outstanding fact is that when one breathes slowly, the ghost remains longer each time, and it is easily observed that the fluctuations in the size of the pupils tend, under these conditions, to occur in the same tempo as do the movements involved in breathing.

It would seem reasonable to suppose that in this particular experiment the peripheral ghosts are more easily obtained than the foveal ones, merely because the peripheral regions of the retinas become affected in a greater degree by each contraction or expansion of the pupils; the illumination of the foveal regions must become thereby only slightly altered, if at all. We have seen that the foveal positive after-form can be revived, either directly or indirectly, by directing the eyes to a white or to a black surface, or by changing the illumination in any other way. An idol-worshiper who has stood in awe of his idol for a long time, might later change the illumination of the retinas properly to revive a positive after-form of his god by looking at a wall, at a cloud, into an open fire-place, or perhaps into a dark well.

C. Cutaneous Ghosts

When we turn to the field of cutaneous sensations, we find positive after-patterns which follow fundamentally the same laws of behaviour as do the visual positive after-images. Periodically recurring cutaneous positive after-effects which appear and disappear in a similar way as do the visual positive ones, are quite frequently observed.[14] We shall discuss as briefly as possible the results of some detailed experiments in this field, and shall attempt to make clear the purpose for so doing by returning occasionally to the visual phenomena.

The subject laid his arm on a table and closed his eyes while I lightly stimulated the fore-arm with a compass-point, the immediate end of which was a nail head with a diameter of 1.6 mm. When this was held at a single place for a long time, the subject made the following verbal responses: "One point," after about twenty-five seconds: "None," after about five seconds: "One," and so on. The periodicity of the recurrence varied with the different subjects, and also with the same individual from time to time. The verbal responses which expressed the periodicity of recurrence, varied for each portion of the arm that was stimulated. As soon as the single nail was one time no longer felt, I applied the second nail of the compass to the skin at a distance of about six centimeters from the original point. The verbal responses heard were: "One, and you shifted it," after three or four seconds: "Two." Then I removed the first point and the subject made no response, so I asked how many points were present, and the reply was: "Two," and immediately: "Now it's only one." Even in this rough experiment, I finally found myself able to predict with considerable certainty at what times the subject would say two points when only a single one was being applied.

Then on the other fore-arm, I applied the two compass points simultaneously for twenty seconds, made a pause of twenty seconds, then again stimulated the same points for the same period, and so on. Finally it was necessary for me to stimulate only one of these points in order to cause both to respond simultaneously and positively. The reaction time of this cutaneous ghost, i. e., the reaction time of the induced response, was apparently no greater than that of the directly produced one, and no instrument has measured this; in all probability it is so very short, that until the contrary

[14] See, for example, Langfeld and Allport, An Elementary Laboratory Course in Psychology, Exp. 19, p. 32.

is proved, we may say it approaches the zero value.[15] (The reaction time of the verbal responses we shall not consider, merely because we have not chosen to discuss verbal ghosts.)

In the last experiment, I applied, after the systematic training, a single nail head, and the subject felt two, but the distance between the two responding areas was seldom accurately estimated; it was generally underestimated, and sometimes the points were judged to be almost together, when the real distance was 5 cm. I applied also the second nail head at a distance of 1 cm. from the first one; and the subject responded just as if the second stimulus had not been applied. This experiment indicates that the presence of the two objective stimuli within the 'Weber's circle' has caused investigators to assert that the individual can be trained to detect their presence there. But the simple fact that a single stimulus is often as good as two, indicates that the threshold does not become reduced as has been supposed.

In order to establish a reaction time for the cutaneous ghost, I chose a decidedly new area on the arm, and applied only one compass point for ten seconds, and then both simultaneously for ten seconds, before I made the regular pause of twenty seconds. I soon found that when I stimulated the first area only, it responded alone for about ten seconds, and then the other area responded suddenly so strongly that, when the second compass point was applied to it, this was a superfluous stimulus for the second area; the area was already responding as if to this stimulus. In this case, the reaction time of the ghost was about ten seconds. This was really a pseudo-reaction time to the stimulus used; the real reaction time was that time which lapsed between the presentation of the stimulus and the occurrence of the most immediately produced response. The second area now responded, not only when it was directly stimulated, but also when the first area became alone directly affected. We must then call the indirectly produced response a cutaneous habit, because it now occurs more frequently than originally. The usual frequency of occurrence is due to the fact that a greater number of the stimuli of the environment produce it, either directly or indirectly. Any response which is produced only directly by a stimulus is an instinctive one.[16]

[15] See my article on 'The Term Reaction Time Re-defined,' which has been accepted for publication by this journal, but which has not yet appeared.

[16] For a more detailed discussion of the problems of instinct and habit, see my articles: Ueber einfache Bewegungsinstinkte und deren künstlichen Beeinflussung; Zeitschrift für Sinnesphysiologie, 1915, and in connection with this, the one on Ubereinandergelagerte Rhythmen bei dem Menschen, in the same journal, 1916.

Fundamentally the same training method can be employed to develop the traditional paradoxical warm- and cold-spots. I stimulated one normal warm-spot with a heated nail head, and at the same time, I applied an ice cold nail to a normal cold-spot for twenty seconds. I then made the usual recuperation pause of twenty seconds, and so on, for an hour. At the close of the period, I found that when I adequately stimulated only one of these spots, the subject frequently judged both to be present. When I asked how far apart they seemed to be, the reply was: "They are almost together." The spots which were stimulated in the training, were in many cases, separated by a distance of about eight centimeters. At another sitting, I stimulated two spots of similar nature alternately, and at the end of the period the subject frequently responded, "Cold," when only the warm-spot was being stimulated. Likewise, the verbal response was often, "Warm," when only the cold-spot was being adequately stimulated. It is interesting that both of these sensations, even more so than the simple cutaneous ones, usually seemed to originate from about the same place on the skin. I later trained two spots in the same way, but by using one cold nail head and one which was at about the same temperature as the skin area of the subject. After the training, the subject reported, "Cold," when only the ordinary cutaneous stimulus was presented to its original spot. This is a paradoxical cold spot produced in the training by using an ordinary cutaneous stimulus and one in which the quality of coldness was emphasized. The cold-response was a negative cutaneous response or a negative cutaneous ghost. The cold-response was the habitual one, the directly produced one was the instinctive one.

We must not forget that we can speak in the same sense of visual instincts and habits. Apparently, in the absence of eye movements, the star gazer may see two or more stars when only one is in the field of vision. According to the results of our cutaneous experiments, we should expect that if the retinas are stimulated by a pattern of stars, this pattern may later be partially or wholly revived when only one of the previously stimulated retinal areas becomes affected by a single star. Also, when the person first directs the eyes to the single star, it might stimulate alternately two retinal areas. Then if the eyes remain still while only one of these areas becomes affected, we should expect a purely subjective or induced star corresponding to the other retinal area to appear and disappear. The observer would accordingly see two stars occasionally. We should expect the induced star

to be more transient than the one for which the objective conditions remain present.

Instead of a constellation of stars, I used a pattern of white spots which were round pieces of paper with the diameter of about 1 cm. I opened the door of the dark-room so widely that everything in the room could be distinctly seen. The observer then carefully fixated two spots, one centrally and one peripherally. These papers were pasted on a black cardboard and were separated by a distance of four centimeters. The distance of the observer's eyes from the spots was about one meter. After a few seconds of careful fixation, I closed the door; and, in the darkness, I placed a black cardboard over the peripheral spot without permitting the subject to know what change had been made. I then opened the door slightly, so that the remaining spot could be seen, and when this was carefully fixated, it was observed to periodically appear and disappear; and many subjects occasionally saw a ghost of the previously perceived peripheral spot. Then, in complete darkness, I uncovered the hidden paper and rotated the cardboard slightly so that the peripheral spot was displaced from its original position by about two centimeters. In the twilight, the observer fixated as usual, and three spots were occasionally seen simultaneously—the two for which the objective stimuli were present, and also the ghost of the previously observed paper. The results yielded by other similar experiments are fundamentally the same as those here reported, and correspond nicely with those obtained by experimenting with the unprotected surfaces of the body.

It may be of interest here to note that for the majority of the observers, the peripherally fixated spot disappears even more readily than the one which falls on the fovea. This occurs even when the eyes are exceedingly well dark-adapted. If, however, a circle of white spots is placed around the foveal one, the central spot, whether fixated foveally or peripherally, disappears, at least for some of the observers, much more frequently than any of those which form a part of the circle. It is often to be noticed that each of the spots induces itself in all directions. Between any two of the circles, the induced effects supplement each other and thus cause a more or less unbroken circle of white to be formed around the central spot. The relatively large area of black which surrounds the central white paper, induces itself in all directions; the white of the small area also induces itself in all directions; and finally there remains no line of demarkation between the spot and its background. When this occurs, we may say the white area dis-

appears. If the white is made larger, it will disappear eventually, but not as readily as when it is small. Of two unequal areas of qualitative sameness, the self-inductive effect of the larger one is greater than that of the smaller one; and, consequently, the larger area does not become as strongly influenced by the self-induced effects of the background. Moreover, the more similar two areas are qualitatively, the more pronounced are the self-induced effects upon one another. It is for this reason that those investigators, who were interested in the absence of the rod processes in the foveal region, and performed their experiments in the twilight with just perceptible white areas, came to the conclusion that the fovea is a blind spot in twilight vision. A white spot, when carefully fixated in ordinary daylight, will disappear, but usually not as readily as in twilight where it is easily made qualitatively more similar to its large background.

The experiments thus far reported show clearly that there is really no fundamental difference between the behaviour of visual and cutaneous ghosts. It seems as if all forms of self-induction are fundamentally the same. Other more elaborate cutaneous experiments which follow, make the similarity between visual and cutaneous ghosts still more apparent.

Instead of the two points of the compass, 315 nail heads were used in training an area on the arm. At distances of 0.4 cm., holes were drilled in a 6.4 x 5.0 cm. block of wood, and the nails were driven into these in such a way that the surface of nail heads received the proper curvature to fit nicely over the arm of the subject. This block was placed on the arm and was allowed to remain for twenty seconds; after a pause of twenty seconds, it was again placed on the arm, and so on, for a period of thirty minutes. After a single half hour of such training, a single nail head of the compass was applied within the trained surface, and the subject reported: "Many, at least a half dozen." A week later, all the effects of the training had disappeared, and after three successive periods, thirty minutes each week, the effects began to persist from one period to another. We have in this case an indirectly produced positive pattern response which is fundamentally the same as any of our visual ghosts. In the case of the nails, the single nail which was applied and caused the pattern response to occur, is analogous to the insignificant illumination of the retinas which revived the elaborate visual pattern. Perhaps not all the 315 nail heads appeared in the revived

cutaneous pattern, and we are just as far from supposing that every single element of the previously perceived visual patterns appeared in the revived ones.

In my article on reaction time, which was previously mentioned, I made brief reference to an experiment similar to the one just described, the only difference being that a smooth surface of wood was used instead of the block of nail heads. After the smooth surface had been systematically applied to a surface of the body for a few days, it was found that a single point, preferably of the same material as that used in the training, so effectively revived the previously perceived cutaneous pattern that a second compass point was a superfluous stimulus when applied lightly within the trained area; the second point was not detected because at least the particular area to which it was applied was already responding as if to this stimulus. In our present experiment a second nail head was a superfluous stimulus when applied to many points within the trained area, because a large number of limited areas within the large one were already responding as if to so many nail heads.[17]

D. Kinaesthetic Ghosts

We shall conclude our ghost theory by discussing some forms of behaviour which are more tangible. A cockatoo moved its foot back and forth near its beak as if it were playing a Jew's-harp in the tempo of 0.21 seconds. I secured a Jew's-harp and played a monotone on it by moving my right hand in the same tempo in which the cockatoo moved its foot. At first the bird would not play with me, but I kept on playing to it occasionally; and finally, for some reason unknown to

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[17] After these experiments on cutaneous sensations were completed, my attention was called to the following interesting, but purely theoretical statement made by William James, Psychology, II, p. 158: "When any point of the sensitive surface has been frequently excited simultaneously with, or immediately before or after, other points, and afterwards comes to be excited alone, there will be a tendency for its perceptive nerve-centre to irradiate into the nerve-centres of the other points." I wish to restate that part of this law which is of primary interest to us as follows: When any point of the sensitive surface has been frequently excited simultaneously with, or immediately before, other points, and afterwards comes to be excited alone in the same way as previously, there will be a tendency for the other points to respond simultaneously with or immediately after this one. It has been reported that a few people can recite, e.g., the ABC's from Z to A as readily as from A to Z without having the special Z to A training, but at no time have I observed such a duplicity of the association in my many human and animal subjects.

me, it began beating in the air with its foot. I ceased playing, but the bird played on. It played a group which contained sixty-one elements (a 61-group), and when it ceased moving its foot, I disturbed it for about a minute by shaking its perch, so that it could not play during the time; it had to clinch the perch fast with both feet to avoid falling. After this pause for the activity in question, I occasionally played the harp, and whenever the cockatoo started playing with me and played the 61-group, I shook the perch for a minute. Instead of bringing about a pause for this unitary act by shaking the perch, I could just as well have fed the beast. Whenever it played anything other than the 61-group, I left the bird unmolested, and it often played again after a short intermission. When it sooner or later played the 61-group, I shook the perch, and only after about three weeks of such training, I found that almost every time I entered the cockatoo's room and started playing the harp, the bird would follow, and in its silent way play the 61-group. I soon found that a minute was an unnecessarily long pause—from thirty to forty seconds sufficed. When I played the harp sooner than thirty or forty seconds after the cockatoo finished the 61-group, it would make either no noticeable response to my music, or, play some other group. Since I did not desire to establish any more than the 61-habit-group, I took special care to make long pauses.

The stimulus which originally started the 61-group was unknown—we shall call it 'X.' The stimulus which later produced the 61-group indirectly was my activity of playing the Jew's-harp—we shall call it 'J.' J produced simultaneously auditory- and visual-responses which were followed by the discontinuous kinesthetic-response, which was the 61-group. We shall assume that X was followed immediately by the 61-group; in other words we shall assume that X was the most adequate stimulus for the initial element of this unitary act. Whenever the 61-group was the direct response to the stimulus X, we may call it an instinct, the elements of which occurred in a constant tempo, a constant direction, and in a relatively constant amplitude of movement, thus making it a unitary group or act. The elements of the group were so many conditioned reflexes; the first was conditioned by X, the second by the first, the third by the second, and so on to the end of the innately associated series. Only those reflexes which are similar to one another are innately associated to form unitary groups of reflexes. Instead of speaking here of association, we might speak of induction, and accordingly say the nervous

correlate of the first reflex of the unitary group was more similar in its chemical makeup to that of the second than to that of the third reflex and consequently induced action in the second structure; and for the same reason, the second then induced action in the third instead of the fourth structure, and so on to the end of the series of the inherently similar structures.

It would certainly be remarkable if X was of such a simple nature that it, when applied to the bird, caused a single reflex, but to avoid argument, let us suppose that this was the case; it makes really little difference what we suppose the very first reflex is like. The structure for the first reflex responded; this structure was similar in various degrees to others and accordingly induced action not merely in a single one, but in a number of others which in turn induced action in their next most similar ones, and so on. We accordingly get a number of qualitatively similar but quantitatively different (occasionally, however, quantitatively the same) groups marching along in the same tempo, and terminating at different intervals. Moreover, new groups may start at any time, e. g., the twentieth structure element or group of simultaneously occurring elements may induce action in the initial element of a new group which none of the preceding elements were capable of phasing. Kymograph records of well trained animals show that groups really do run in just such a temporally superimposed order; and these records also show us that when a certain number of these groups run their natural or innately determined courses simultaneously, many, and in cases apparently all of the remaining ones cease. If some of them continue, the beating member begins to move in a slightly different way, perhaps in a different direction, tempo, or general amplitude of movement; and, without waiting for a pause, we accordingly begin to count a new group which continues until a certain number of these cease simultaneously. Since such qualitatively similar groups begin and terminate at such irregular intervals, and since all of them are expressed by the same beating member, it becomes clear, for this if for no other reason, why the amplitude of movement of a given member always varies—even though the fluctuations may be slight—while the tempo and direction of movement may remain extremely constant for a considerable time.

Even in such a simple case as the one just discussed, we by no means observe a simple unitary group, but a great number of qualitatively similar ones which might be produced simultaneously by a properly designed pattern of stimuli. If

in the case of the 61-group such a thinkable pattern should be presented, all the groups that have concerned us would start simultaneously; and, the general amplitude of movement must necessarily become less as the quantitatively different groups terminate one after another and occasionally several at the same time. Such a phenomenon of fatigue, we observe in the field of visual sensations when the retinas become affected for only a few seconds with an intense visual stimulus. If the psychical state produced is one of whitishness, it becomes gradually less and less distinct.

We have supposed that the primary effect of the stimulus X is the first reflex, i. e., the first beat the bird executed, and the remaining sixty beats then compose the positive after-effect or the kinaesthetic instinctive ghost. Now, if we can revive the entire 61-group by using some stimulus other than X, we may speak also of the kinaesthetic habitual ghost. The stimulus J serves this purpose. We may now speak more generally and say the macro-61-group is a kinaesthetic pattern conditioned by micro-visual and auditory groups. As is the case with the visual and cutaneous ghosts, we also find it impossible to produce the kinaesthetic one while the structure correlates are in a state of exhaustion; we must in all cases wait until an adequate recuperation pause for the structures has elapsed.

Our reasoning concerning the innate associations of reflexes would lead us to suppose that when the 61-group became a habit, by virtue of the fact that it became associated with at least one other group that appeared frequently, the structure correlate of its first element and that of the last element of the conditioning group became chemically more similar than originally. Without stopping in this paper to discuss a theory of how this might take place, I shall dismiss the subject for the present by merely suggesting that, if the structure correlate of the first element of the 61-group is stimulated to function while the last element of the micro-group (in this particular case a micro-group) is still in a state of dissimilation, a part of the former may become included in the latter substance.

PSYCHOLOGICAL TESTS FOR THE AUTHORSHIP OF THE BOOK OF MORMON

WALTER FRANKLIN PRINCE, Ph.D.

The application of rigorous psychological tests to the mind of a man long since dead has its interest, and if thereby light is thrown upon a question of perennial debate, it achieves practical importance. The man to whom such tests will be here applied is Joseph Smith, the founder of Mormonism, who was killed in 1844, and the question which will be illumined is that of the authorship of the Book of Mormon.

There are three theories as to the origin of that book.

(1) It is a translation of the gist of records made by a succession of scribes of peoples anciently inhabiting America. This is the belief of several hundred thousand persons who find in the book the chief distinctive source of their religion. Since the odd contents of the volume lamentably or ludicrously fall before every canon of historical criticism, scholars have not thought it worth while to discuss the notion of its ancient authorship, unless briefly for pragmatic and missionary purposes.

(2) It was in the main written by Rev. Solomon Spaulding, who died in 1816, as a romance, but some religious matter was added by Joseph Smith, solely or with the assistance of Sidney Rigdon. This has been the prevailing view outside the ranks of its religious devotees, since about 1830.

(3) It was solely or essentially the work of Joseph Smith himself. This is maintained by a few scholars, mostly within the last 15 years. Prolonged analysis and comparison by the present writer make it incredible that Spaulding had any connection with the book, doubtful that Rigdon was implicated, certain that Joseph Smith's hand is perceptible in every part, and probable that he was the sole author, the edifice of whose imagination echoed to reminiscences which he was far from recognizing.

Three propositions may firmly be laid down, the evidence for which has never adequately been set forth, and, except for a part of that under the third head, finds no place here.

(A) If there were no knowledge of Joseph Smith what-

ever, or of the date when the Book of Mormon was copyrighted, it would nevertheless appear, from the numerous reflections of the times which it contains, that it was written somewhere between 1820 and 1834. Many passages certainly could not antedate 1826. With what we know about Smith and the copyrighting of the book, we are able to narrow down to the period between 1826 and 1829, with emphasis upon the year 1827. (Spaulding died in 1816.)

(B) If there were no knowledge of Smith, it would yet be most probable that the author lived in western New York. (Smith did, but Spaulding and Rigdon did not.)

(C) Having in possession our meagre knowledge of Joseph Smith's early career, and of his mental traits, all the assignable data in the Book of Mormon point to him and him alone as the author. For example, several of the dreams and visions contained in the book are incontestably slightly altered versions of dreams experienced by his father, which we find guilelessly related long after the prophet's death, in his mother's reminiscences.[1]

The tests which we are to apply are concerned mainly with the proper names in the Book of Mormon. The principle upon which they rest is found in the influence which memory-and-emotion complexes exert upon the invention of combinations of consonantal and vowel sounds. If a man is spinning a tale of fiction and manufacturing therefor quaint personal and geographical names, it is not the case that one combination of sounds will enjoy an equal chance with another of emerging in his consciousness. On the contrary, a combination resembling what may be called a master-word associated with some oft-repeated or strongly emotional experience of his past life will be much more likely to offer itself to his mind than any combination not so associated. For instance, if he has formerly been bitterly injured by a woman named Caroline, or else fondly loved one of that name, so that it is deeply imbedded in his memory and invested with strong feeling, it is many more times as likely that a combination beginning with "Car" will present itself to his mental view than that the immune syllable *bar, dar,* or *far,* etc., will do so.

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[1] After tracing these passages in the Book of Mormon to their unmistakeable archetypes in Lucy Smith's story, I found that this had already been in part done by I. Woodbridge Riley, in his excellent "Founder of Mormonism," printed in 1902.

An honest elder of the Reorganized Mormon Church, to whom the "deadly parallels" were pointed out, could not deny that they existed, but suggested that the elder Smith might have had dreams prophetically forestalling the discovery and translation of the "Golden Plates"!

In fact, the whole name "Caroline" will tend to suggest itself. But then, if the authorship of the fiction, or the fact that it is fiction, is to be concealed, the too tell-tale word, emerging into the upper consciousness, will be rejected. But if it comes up, as it will then tend to do, in an altered and disguised form, as *Carlin,* or *Carowin,* and is not recognized, it is likely to please the unwary consciousness, as by a thrill, and be accepted.[2] If the inventor of names is of a strongly emotional and imaginative type, and especially if he approach the abnormal in this respect, the tendency will be pronounced. Since it is certain at least that Joseph Smith was responsible for the incorporation of his father's dreams into the Book of Mormon, it is not premature to remark that he was thus characterized.[3] At any rate, the author of that book was, as will be shown, demonstrably subject to the tendency, which will betray, as we proceed, first that he probably lived, like Joseph Smith, in western New York, secondly that he invented many of the names within three or four years before Joseph Smith offered the book for copyrighting, and thirdly that he was either Joseph Smith himself or a man many of whose personal antecedents and relationships duplicated those of Joseph Smith to a degree unheard-of and incredible.

Entering upon our thesis, it is first necessary to set forth one of many reflections in the Book of Mormon of the times in which it was written, since the emotional accompaniment in the mind of the author furnished the soil out of which a throng of the invented proper names grew. I refer to the Anti-Masonic excitement which began with the abduction of William Morgan in 1826. This man had announced that he was about to publish a full account of the secret rites and alleged tendencies of Masonry, and having been arrested in Batavia, New York, on a charge of theft, was taken to Canandaigua and there acquitted, rearrested for debt and lodged in jail, and thence taken to the Canada line where all traces of him finally disappeared. The agents in these acts appear to have been Masons. The popular excitement roused, beginning in western New York, was prodigious. The feeling first in

[2] If the fiction, and consequently the names, should be a semi-conscious or subconscious and automatic construction, as may very possibly have been the case with the Book of Mormon, the mechanism involved would not be dissimilar to that stated. The associational and emotional processes would still govern, and identical names which would constitute a "give-away" would be rejected by the inner "psychic censor," to adopt a Freudian term, while slightly disguised ones might pass its inspection.

[3] See Riley, "Founder of Mormonism."

western New York crystallized into a political movement which spread more or less over the whole country as the Anti-Masonic Party. The movement rapidly subsided, and even in New York the party ceased to be a positive factor in 1833, but feeling still continued to be strong in the western part of the State, where Smith lived.[4] The Morgan pamphlet was printed after his disappearance, and we shall presently see that the writer of the Book of Mormon was familiar with it.[5]

Now in at least twenty-one chapters in seven out of the sixteen "books" of the Book of Mormon are to be found passages, varying from several to sixty-three lines in length, plainly referring to Masonry under the guise of pretended similar organizations in ancient America. The warning of Washington in his Farewell Address, against "*combinations* . . . with real design to direct, control, counteract or awe the regular deliberation and action of the constituted authorities" was quoted a thousand times in Anti-Masonic speeches and writings, and accordingly we find the Book of Mormon employing the term "*combination*" five times in its descriptions of the alleged ancient societies, and "*secret combinations*" fifteen times. Thrice it boldly names them "*secret societies*," while "*secret works*," "*secret abominations*," "*secret plan*," "*secret signs*," "*secret band*," "*secret oath*," and "*secret words*" are employed *ad nauseam*. The claim or poetic fiction of the Masons that their order is from very old times is reflected in "which had been handed down from Cain."[6] "They did have their . . . secret signs and their secret words, and this that they might distinguish a brother"[7] has a familiar sound, even to the word "brother." Once the word "craft"[8] is employed in this connection, not only a word in technical use by the Masons but also found on the title-page of the Morgan pamphlet. No charge was

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[4] "Life of Wm. H. Seward" by E. E. Hale, Jr., pp. 69, 71, 104. See also "W. H. Seward" by T. K. Lothrop, pp. 15-16; "Horace Greeley" by James Parton, pp. 101-102; MacMaster's "United States," vol. V; "Roberts' "History of New York," pp. 580-581; contemporary pamphlets and periodicals.

[5] The essential part of the title-page reads thus: "Morgan's Masonry exposed and explained, showing the origin, history and nature of Masonry, its effect upon government and the Christian religion, and containing a key to all the degrees of Masonry . . . the whole intended as a guide to the craft and a light to the unenlightened." By Captain William Morgan.

[6] Book of Mormon, Ether 8:15. All references to the B. of M. are to the Utah edition of 1908.

[7] B. of M., Helaman 6:22.

[8] B. of M., Helaman 2:4.

more frequently sounded in the furor of 1826-33 than that the Masons monopolized the offices, and defeated justice in the courts in the interest of their members, and accordingly we read in the Book of Mormon of the "secret combinations" "filling the judgment-seats, having usurped the power and authority of the land . . . letting the wicked go unpunished because of their money, and moreover to be held in office at the hand of government, to rule and do according to their wills, that they might get gain and glory of the world; and moreover that they might the more easy commit adultery, and steal, and kill, according to their own wills."[9] Innumerable papers and pamphlets declared that Masonry was subversive of freedom and popular government (and this is intimated on the title-page of the Morgan pamphlet), and so the supposedly ancient record sagaciously speaks of "this secret combination which *shall* be among you" and warns that "whosoever buildeth it up seeketh to overthrow the freedom of all lands."[10] As Masonry was charged with being inimical to religion (and this also is intimated on the title-page of the Morgan pamphlet), so we find the replica "They did reject all the words of the prophets, because of their secret society and wicked abominations."[11] But, more pointedly, not only are the general charges against the Masons faithfully impressed upon these many passages of pretended ancient date, but so also is the tragedy of William Morgan. Twenty-eight times, and in almost every passage, are the "secret combinations" coupled with "murder" and "murderers," while the words "kill," "slay" and "blood," with similar implications, are employed. The source of the obsessing idea becomes more patent with the four-fold use of the expression "secret murder,"[12] since Morgan was murdered secretly if at all. Even the killing of a Book of Mormon character in "a secret pass" is probably a reflection of the belief that Morgan was drowned in the clandestine *passage* from the United States to Canada. At any rate, it is impossible to mistake the connection between the belief of the masses that the light sentences of the several men convicted of Morgan's abduction was an insult to justice and the statement in the Book of Mormon that lawyers and others connected with the ancient covenants conspired to "deliver those who were

[9] B. of M., Helaman 7:4-5.
[10] B. of M., Ether 8:24-25.
[11] B. of M., Ether 11:22.
[12] B. of M., Alma 37:22; Helaman 8:4; Helaman 6:29; 3 Nephi 5:5.

guilty of murder from the grasp of justice."[13] And parallels continue. It was charged that Morgan was practically condemned in a secret session of a lodge, and as a matter of course the pretended record declares it the case in ancient America that "whosoever of those who belonged to their band who should reveal unto the world of their wickedness and their abominations should be tried not by the laws of their country but by the laws of their wickedness."[14] Here is a double parallel, for the illegal condemnation in both cases was for exposing the secrets of the order. But as the modern crime was in vain, since the Morgan pamphlet was published nevertheless, so in the Book of Mormon we hear the exultant cry, "Their secret abominations have been brought out of darkness and made known unto us."[15] To fairly cap the climax, the widow and babies whom Morgan left finds her parallel in the "widows"[16] and "orphans" of the Book of Mormon, made such by "secret abominations"; and the fact that the murderers of Morgan (if he was indeed murdered) never were punished, is reflected in the sentence, forming part of a paragraph about the ancient "combinations," "The Lord will not suffer that the blood of his saints [!] which shall be shed by them shall always cry unto him from the ground for vengeance upon them."[17]

And now we plunge into *medias res*. It is now sufficiently evident that the author of the Book of Mormon was, at the time he was writing it, powerfully obsessed by the ideas and emotions which characterized that popular movement which, beginning in western New York in 1826, was to subside last in the same region. What word would sink most indelibly into such a consciousness—what but the name MORGAN itself? Over and over again, as the writer sought a name for a new character or locality, the name Morgan would present itself. But this telltale name would be rejected, by the upper intelligence if the work was conscious fiction, by the "psychic censor" if it was "an automatic product." But when the word came up in a disguised shape, the first syllable "*Mor*" intact, the letter "m" either elided or substituted, a vowel either the same as the second vowel of the obsessing name or similar to it in common utterance, the identical letter "*n*" following, with or without additions, an unsophisticated upper intelli-

[13] B. of M., 3 Nephi 6:29.
[14] B. of M., Helaman 6:24.
[15] B. of M., Alma 37:26.
[16] B. of M., Mormon 8:40.
[17] B. of M., Ether 8:22.

gence or subliminal "psychic censor" would not perceive its betraying quality, and it would be written down. Now in the book which we are inspecting there are not fewer than twenty-five words which begin with the syllable "*Mor,*" and every one of them is presently followed by the letter "*n,*" with either the identical vowel "*a*" preceding it, or the vowel "*o*" (and in popular speech the pronunciation of *o* in *Mormon* and of *a* in *Morgan,* are practically identical, being quite or nearly equivalent to the short sound of *u*). Also, precisely as "Morgan" is the masterword of the particular ideational and emotional complex of which we have been speaking, so *Mormon,* one of the reflected names, is the chief character of the composition, while "*Mormon*" is also the name of the composition as a whole. The entire list follows.

MORmoN (Lamanite king)
MORmoN (son of the above)
MORmoN (Nephite prophet)
MORmoN (name of entire book)
MORmoN (division in Book of Mormon)
MORmoN (name of a forest
MORmoN (land of)
MORmoN (place)
MORmoN (body of water)
MORmoN, Words of (division in Book of Mormon)
MORoN (Jaredite king)
MORoN (land of)
MORoNi (Nephite prophet)
MORoNi (last Nephite)
MORoNi (division of Book of Mormon)
MORoNi (city)
MORoNi (land of)
MORoNihah (Nephite general)
MORoNihah (another Nephite general)
MORoNihah (city)
MORiaNton (founder of city)
MORiaNton (Jaredite king)
MORiaNton (land of)
MORiaNton (land of)
MORiaNtom (land of)
MORiaNcumr (place)

Now, while the fact that out of the 40 proper names in the Book of Mormon having the initial letter M, 25 begin with the syllable *Mor,* and the fact that every one of these 25 further contains or approximates the final two letters of the obsessing name "Morgan," are impressive, it is not expected that they will be convincing by themselves. The demonstration is but begun. And right here we add that there are at least two other reflections from the same name (making a sum of 27), namely:

AmMORoN (Nephite apostate), and aMORoN (Nephite officer).[18]

For some time I stupidly wondered why the writer made

[18] It is worth considering whether the wraith of *Morgan,* appearing in Ammoron, has not brought along with it the echo of the last syllable of *William.* Compare

williAMMORgaN and
AMMOR oN

so many of his proper names begin with the syllables *"Anti,"* suspiciously identical with the *Latin* prefix. But suddenly it dawned upon me. What word connected with the excitement of 1826-33, rivalled "Morgan" as a tocsin-call to the emotions? Manifestly, "ANTI-MASONIC," the name of the political party which Morgan's abduction roused into being. Consequently, we find the distinct reflex of the prefix in 14 proper names of the Book of Mormon:

ANTInephilehi (a people)
ANTInephilehi (Lamanite king)
ANTIomno (Lamanite king)
ANTIonah (a ruler)
ANTIonum (Nephite general)
ANTIonum (land of)
ANTIparah (city)
ANTIpas (mountain)
ANTIpus (Nephite commander)
aniANTI (village)
mANTI (person)
mANTI (city)
mANTI (land of)
mANTI (hill)

The obsessing prefix clamored incessantly for deliverance, and achieved it, but not perfectly, in 14 other instances:

archeANTus (Nephite officer)
coriANTon (son of Alma)
coriANTor (father of Ether)
coriANTum (Jaredite king)
coriANTum (Jaredite prince)
coriANTum (Jaredite captive)
coriANTumr (Lamanite general)
coriANTumr (last of Lamanites)
ANTum (land of)
gadiANTon (a robber chief)
irreANTum (body of water)
moriANTon (land of)
moriANTum (land of)
seANTum (a nephite)

But why should not the latter member of the term "Anti-Masonic" be reflected among the names in the Book of Mormon, as well as the former? So the reader may ask, and so the writer asked, and looked, and behold it was, in

MAthONI and MAthONIhah

Just lisp the sibilant and you have the entire word "Mason" and almost the entire word "Masonic" in both of these appellations. Does this only happen so? Then why does there not happen to be in the list a single Bathon, Cathon, Dathon, Fathon, Gathon, Hathon, Jathon, Kathon, Lathon, Nathon, Pathon, Rathon, Sathon, Tathon, Vathon, Wathon, or Zathon? Not one of these is more unlikely in itself than certain names which do occur, as "Zeezrom." But it is precisely "Mathon" which we find, and two of them at that.

This brings us to a resembling group of odd names.

Zeezrom (a corrupt lawyer)
Zeezrom (a city)
Cezorum (leader or tool of robbers)
Seezorum (judge elected by robber band)

Each of these names commences with the soft or hard sibilant, each is followed by sounds which may be variously spelled

"eez," "ez," or "ese," and each continues with "ro" or the same letters reversed in order. It seemed to the present writer that they also must have risen from some obsessing word which persisted in offering itself, and was accepted four times when slightly disguised. Among the names of the men arrested for Morgan's abduction I found that of one Chesebro. This name resembles those of the above group in several particulars. The initial sound is that which most resembles the sibilant. The combination "ese" is the phonetic equivalent of "eez" and "ez." And, disregarding the "b," "*ro*" is found in two of the group, and in the other two in reversed order. Compare CHESEbRO and ZEEZROm.

But why should the name of this man have produced an emotional impression more than the names of the other three men convicted,—Lawson, Sawyer and Sheldon? For no reflections from the latter three can be found. Research disclosed that Chesebro was the principal actor and spokesman in the proceedings against Morgan up to the time that he was taken from the Canandaigua jail and hurried to the Canada line.[19] Chesebro was the man who obtained the warrant for Morgan's arrest on the charge of theft. Chesebro raised the posse that went to Batavia after him. Chesebro is almost the only one mentioned as speaker at the time of leaving Batavia, in the various affidavits. Chesebro had another legal paper ready so that when Morgan was acquitted in Canandaigua he was rearrested for debt and put in jail. Chesebro appears to have been behind the proceedings by which Morgan was delivered from jail and started on the way to Canada. Chesebro's sentence was lighter than that of Lawson, because he was to a degree sheltered by legal forms, and although Lawson had the most prominent part in the final journey, and Chesebro drops out, the later incidents remained in comparative obscurity. And let it be noted that Canandaigua is but nine miles from Manchester, the home of Joseph Smith, who if not there during a part of 1826, without question lived there after his marriage in January, 1827, when the excitement was at its height.

Finally, in this connection, the name Archeantus, already cited among those affected by the "Anti" prefix, probably reveals another relationship. It was charged that the actual drowning of Morgan was accomplished directly after the in-

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[19] "William Morgan, or political Anti-Masonry," Rob. Morris. "Republican Advocate," Batavia, N. Y., issue of Sept. 29, 1826. "Narrative of the facts and circumstances relating to the kidnapping and presumed murder of William Morgan," Rochester, 1827.

stallation of a Royal *Arch* chapter, by persons who took part therein. Also that Morgan himself became a Royal *Arch* Mason a year or two before his supposed death. Thus we find *anti* and *arch* appropriately conjoined in "Archeantus."

Thus far, we have found the one Morgan-Antimasonic complex (the same which in after years subconsciously influenced Joseph Smith to call his fiscal institution, on the wildcat bills which it issued, an "*Anti*-Banking Company") influential in the production of 59 names out of the about 350 in the Book of Mormon, or more than a quarter of the whole list, aside from those either taken intact from the Bible (77) or transparent imitations of Biblical names (upwards of 50).[20]

As has been said, in spite of the abduction of Morgan his pamphlet, professing to disclose the ritual of the first three degrees of Masonry, was published. Later in the same year, 1827, someone followed it with a pamphlet revealing the ritual of the four next higher degrees. Still later the two pamphlets were issued as one, and are so reprinted to-day. Now the author of the Book of Mormon was familiar with the Morgan pamphlet but not with the other. The proof of this double assertion is found in the fact that in almost every case where, in the Morgan pamphlet, a word is capitalized or italicised because of its technical employment in the ritual, the word is found in a slightly disguised form among the proper names of the Book of Mormon, besides other reflections, while no such reflections from the second publication are discernible. Anyone can consult the reprint for himself, and test the truth of the following assertions.

We do not find "Tubal-Cain" (italicised on pages 55, 59, 69 and 80 of the Morgan pamphlet) in the Book of Mormon, but we do find TUBALoth.

We do not find "shibboleth" (italicised on pages 39, 40, 43, 45, 46, 49, 53 and 79), but we do find SHIBLom (two applications) and SHIBLon.

We do not find "Jachin" (capitalised on pages 43 and 79, and italicised on pages 43, 46 and 52), but we do find JACom and JAshoN.

We do find "Boaz" (capitalised on pages 21 and 79, and italicised on pages 31 and 52), since the fact that it is a Biblical name caused it to pass unaltered.

We do not find "Mahahbone" (capitalised on pages 80 and italicised on page 64), but we find MAHAH.

We do not find "Hiram Abiff" (italicised on page 61), but we find ABIsh.

[20] See "Dictionary of the Book of Mormon," Geo. Reynolds.

On page 52 of the Morgan pamphlet there is an allusion to the (mythical) Palestinian city of "Zaradatha." There are no italics this time to make this name stand out, but its own sonorous, mouth-filling magnitude was probably as effective, besides which the purported city is mentioned in the course of a paragraph which, as we shall see, for other reasons strongly impressed the writer of the pseudo-history. The chief city of the Book of Mormon is not called Zaradatha, but it is called ZARAhemlA,—the same first two syllables, the same termination, only three letters in the same total of nine altered, the same number of syllables. Who can doubt the relationship of the two artifects?

The two "Jonases" in the Mormon set of twelve disciples is probably reminiscent of the fact that the Masons dedicate their lodges to John the Baptist and John the Evangelist, as stated on page 39 of the Morgan pamphlet; and "Jonas" is suggestive of *Johns,* aside from there being two of the former.

An amusing instance, and one of the most significant, is the name "Isabel," one of the three feminine ones in the Book of Mormon. With the illiterate writer, indeed, there was nothing about this word to put either his upper consciousness or his interior "psychic censor" on guard. But to us the Old French and Spanish name Isabel is richly grotesque considered as that of a descendant of Israelitish stock living in America some 2,000 years ago. The source of its adoption is clear almost to demonstration. For reasons which cannot here be set down it appears that the writer was familiar with some book giving a meagre account of the first voyages to America and a very elementary description of the so-called civilizations found there, in Peru, Central America and Mexico. What woman was bound to be mentioned, far more prominently than any other if not exclusively? Manifestly Isabella, the Castilian queen who financed Columbus's memorable first voyage to America. The name Isabella, insistently knocking for admission through the avenue of the writer's mind into the Book of Mormon, would be rejected, but under the slight disguise of "Isabel" it effected entrance.[21]

[21] Isabel (B. of M., Alma 39:3-4) is represented as a harlot. This too may be a result of associative processes in a dreamily reminiscent mind: (1) The author was familiar with the ultra-Protestant view identifying the Roman Church with the "scarlet woman," and the "harlot" of the Revelation of St. John. (2) Isabella being a Roman Catholic, and the Spanish Inquisition having been founded during her and Ferdinand's reign, the concepts of the "scarlet woman" and of Isabella tend to coalesce; (3) the name Isabel successfully emerging in his mind, it draws after it the notion of harlotry.

Before parting with the Morgan pamphlet utterly, we may clinch its connection with the Mormon scripture by reference to several matters not concerned with proper names. In the latter is the curious incident of Lehi's finding at the door of his tent (1) a ball, (2) made of brass, (3) hollow, (4) having inside, (5) two spindles, (6) one of which persistently pointed the way that should be traveled.[22] A brass ball, with spindles *inside,* seems a curious sort of an arrangement for even a miraculous compass, neither is it evident from the narrative why there were *two* spindles. But turn to page 52 of the Morgan pamphlet and all is clear. There we find it declared (though the information is not authentic) that on each of the two great pillars in Solomon's Temple was (1) "a large globe or ball," that it was (2) of "brass," that it was (3) "hollow," and that (4) inside were (5) two sets of maps, one of the celestial, (6) the other of the terrestial bodies. We see now why the spindles of Lehi were inside the brass ball, this is a mere, unreasoning reminiscence of something being inside the pretended ball of the temple. And we see why there were two spindles, there were two sets of maps in the archetypal ball. One of the spindles was certainly a good substitute for terrestial maps, as it pointed exactly the way to go, while the other is reminiscent only, without its use being explained.

Again, Nephi, on a certain occasion, stretches forth his hand, and his brothers experience *"a shock."*[23] We need only to cite that on page 27 of the Morgan pamphlet it is stated that at one stage of the opening of a lodge the members stamp and clap their hands at the same instant, and that this is called (italics in the original) *"the shock."*

The exhibition thus far of the psychical mechanics involved in the invention of names for the Book of Mormon has been of interest, but has afforded only stray, though significant, indications that the writer was Joseph Smith.[24] Unfortunately

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[22] B. of M., 1 Nephi 16:10.

[23] B. of M., 1 Nephi 17:53.

[24] There are many indications of a sort not pertinent to this paper of Smith's authorship. In fact Joseph can be seen at various intervals, walking through the volume. As the dreams of Lehi, father of Nephi, repeat the substance of the dreams of Joseph's father, so Nephi himself in his relations with his unbelieving as well as his believing brothers, probably stands for Joseph and his brothers with their differing views regarding his early claims. Over and over again passages reflect the known environment of Joseph Smith. Facts personal to Joseph are found. For example, he is known to have practiced "crystal-gazing," and in the Book of Mormon more than one mention of a transparent stone which revealed hidden facts, is to be found. To all but believers in the authenticity of the book as a record of ancient American peoples,

our knowledge of his early experiences of strongly emotional cast is but scanty. But we do know that he was forming an attachment for a certain girl in 1825, and that he married her Jan. 18, 1827. Since the book in question contains scattered throughout it passages and names which reflect the Anti-Masonic excitement, it could hardly have got on very far before 1826, and it must have been finished by 1829, for in that year it was copyrighted. The period of Smith's courtship and early married life corresponds, then, pretty closely with the period when the Book of Mormon was writing. Since no name is more vividly engraved on the mind of a young man than that of the girl of whom he is enamored, if Joseph Smith wrote the book and if our thesis in regard to the influence of emotional complexes upon its proper names is correct, we ought to be able to find the first name of his sweetheart, Emma, and very likely the last name also, Hale, in penetrable disguise. We do not find "Emma," nor would we expect to do so, for the "psychic censor" above or below the threshold would not let it pass. But we do find both "*Emer*" and "*Ammah*," the two proximate substitutes. We do not find "Hale," though that must have clamored for emergence, but it repeatedly succeeded in securing adoption unrecognised by the device of exchanging the vowels. Thus we have

HELAm	(Separate and	HELAman
HELAm	distinct persons	HELAman
HELAm	and places)	HELAman
		HELAman

If these correspondences are accidental, it ought to be as easy to find correspondences in the cases of names hit upon haphazard, or taken *en masse*. For example, I will produce the names of all my living near and feminine blood-relatives (not to make the list too long), and anyone interested may compare them for himself, if he will take the trouble, with the list of names in the Book of Mormon. First names: Elmira, Louise, Clara, Ella, Cora, Imogen, Maude, Nellie, Mabel. Last names: Prince, Blackman, Graves. There is one slight resemblance that of Louise to Luram and one stronger one, that of Cora to a group of names beginning

with all its glaring departures from historical possibilities, the prediction that there would arise a seer who should be Joseph Junior and do various things that the actual Joseph Junior professed to have done, and the prediction of a peculiar accident which happened in the course of the "translation" of the Golden Plates causing Joseph considerable discomfort, point to him as author.

"Cor—." But of course out of a considerable number of names taken at random or *en masse* occasional accidental resemblances are bound to occur. It is where we are able to go to the spot and say, if this principle which we appear to have discovered is really valid, such a name should find a resemblance, and the predicted result over and over again follows, that the correspondences are convincing. We happen upon accidental occurrences now and then, but science, the knowledge of governing law, foretells occurrences and where they shall be looked for. Take the population of the earth and you will occasionally find a man who paints a turtle as a symbol in some way personal to himself, but ethnological science says, if that particular man belongs to such an Indian tribe he certainly paints a turtle upon his cheek, as the totem-mark of his nativity. Moreover the list of my female relatives did not disclose accidental resemblances between both first and last names of a single individual and Mormon names, whereas both Emma and Hale find their correspondences, as was most unlikely to occur by chance, but almost certain by operation of the psychological law which has been explained.

Again, one would naturally predict that if any of the numerous towns which Joseph Smith lived in prior to 1826 were to be found reflected in the names of the Book of Mormon, Sharon where he was born, and Manchester which was his residence from his 13th to his 20th year, and again after his marriage, would be the ones, rather than those which boasted of him for short periods between the time of his birth and his 13th year. And we do find SiRON, and

MANti (Nephite spy)	
MANti (city)	also four HelaMANs
MANti (land)	and six LaMANs.
MANti (hill)	

One may object that the Mantis have been already accounted for by the obsessing "Anti." This is true, but the two derivations do not exclude each other, but on the contrary furnish a good example of the processes involved. Having got as far as the *Man* suggested by the name of his own town, the reminiscent consciousness of Smith found itself already in the *anti* groove, and completed the short journey.

It appeared to the present writer, by this time, almost certain that the name Harmony, that of the town where Joseph Smith spent so many happy, loving hours courting Emma, would be discernible, so he again consulted the list and found HiMNI. I need not point out the radical resemblance. Is

that resemblance accidental, and not due at all to the haunting cadences of that doubly-blessed name "Harmony?" Let us again test the theory of accident by my own relatives, who certainly had no part in the authorship of the Book of Mormon. My father got his girl in Detroit; if there is any name in the Book of Mormon which strongly resembles Detroit it will certainly be an accident. The only two Mormon names beginning with D are David and Desolation. One of my uncles got his girl in Palmyra; there is nothing nearer than Pahoran. My other uncle got his girl in Pittsfield; there is nothing nearer than Pacumeni. My older brother got his girl in Moxie; Mocum is the nearest resemblance. I got my girl in Newport, which matches best with Nehor. My younger brother got his girl in Detroit, which finds only David and Desolation to be compared with. This is not a selected list, for I know not where another male relative got the partner of his joys and sorrows. But here are six cases in any one of which a decided resemblance *might* turn up by chance, over against one case where the decided resemblance was looked for in obedience to law. A shadowy resemblance or two may indeed be fancied between members of the group and names in the Book of Mormon, but surely nothing comparable with this

HarMoNY HiMNI

The two emotional experiences of which we know, in Joseph Smith's early life, were exceedingly fruitful of effects upon the invention of proper names in the strange book which must have been his composition. Were we informed of other such experiences, we could doubtless trace their effects also. One such, we are reasonably sure, existed, to account for the first two syllables in a group of names already casually referred to. This is the list.

Corianton (son of Alma)
Coriantor (father of Ether)
Coriantum (Jaredite king)
Coriantum (Jaredite prince)
Coriantum (Jaredite captive)
Coriantumr (Lamanite general)
Coriantume (last of the Jaredites)
Corihor (Jaredite prince)
Corihor (another Jaredite)
Corihor (land of)
Korihor (Nephite anti-Christ)

Here are eleven names in every one of which the first two syllables are pronounced exactly the same. If the author was, before he met Emma, we will say, in love with a girl named Cora, that might account for it. If a man named Corey was the center of a strongly emotional experience, that would account for it still better. We do not have the data to de-

termine what the experience was which set the combination "Cori—" so frequently knocking, though pretty certain that there was one.

Was there a discernible tendency to apply obsessing words, in disguised form, with discrimination, attaching those of agreeable association to persons conceived of as good, and those of disagreeable connotation to those regarded bad, in the Book of Mormon? It would seem so, the tendency apparently acting in dreamy fashion, and hence not always accurately. For example, Morgan was regarded as a martyr, dying for the sacred cause of light and liberty, hence to be considered as crowned with the pleasing halo of goodness. Of the eleven mythical persons whose names are reflections of his, eight appear to have been good and three bad. On the contrary, Chesebro, as the persecutor of Morgan, was regarded as a fierce, disagreeable personage. Accordingly Cezorum was a robber belonging to a "secret combination;" Seezorum was a corrupt judge, member of the same order; and Zeezrom was a wicked lawyer (note that Chesebro was the one of Morgan's abductors who sheltered himself under legal warrants) who withstood the servants of God. Emma and Hale were certainly names invested with agreeable associations, and all the characters, seven in number, whose names are reflections from one or the other of these, appear to be good. Harmony, as the place of Joseph's love-making, was surely one of charm, and we are prepared to find Himni an exemplary gentleman. At first it might appear that Mathoni and Mathonihah, since *Masonry* is so dreadful, should have been bad men, but then we remember that "Anti-Masonic" was the obsessing word, and Anti-Masonry was regarded as irreproachable.

There is also somewhat of a tendency in the names derived from a particular complex to group themselves together in the Book of Mormon narrative. For example, Ammoron is most prominent in connection with a correspondence between him and Moroni, and is killed in the city of Moroni, which is in the land of Moroni; while Amoron is mentioned only as the giver of certain information to Mormon. No attempt has been made to work out further groupings.

We have already seen that Joseph Smith in after years manifested one of the obsessions so visible in the Book of Mormon, when he gave his Kirtland fiscal institution the singular name, "Anti-Banking Company." Moroni is also one of the betraying names of the Book of Mormon, but it is outside of its covers that we are told of the resurrected

Moroni, who led the way to the Golden Plates. It is certainly the mouth of Joseph Smith now that utters the name of veiled significance in connection with his own alleged adventures. It is not in the Book of Mormon, but in one of those subsequent "revelations," which, if Deity did not compose them, Joseph Smith did, that Joseph is given a new name.[25] This name, Gazelam, is of double significance: first because it was applied in the Book of Mormon to a man who was given a stone in which to see hidden things, exactly as Joseph himself had been accustomed to seek for knowledge by means of a "peep-stone;" and secondly because the very word "Gazelam" contains unconcealed another word expressive of the process by which knowledge was thus sought.

And best of all, it is in one of Joseph Smith's "revelations" that we are informed that the name of a character unnamed in the Book of Mormon, and undesignated other than that he was "the brother of Jared," was really Mahonri Moriancumr. Now that we have the keys to his ruling complex, he might as well have written that the name was

MASONRY MORGAN.

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[25] Smith's "revelations" likewise gave new names to some of his living associates, and to things. In some instances the mechanism so apparent in the Book of Mormon is discernible here, as when he renames Oliver (Cowdery) *Olihah,* and the tannery *tahhanes.* But in other cases there is no resemblance, probably because the "psychic censor," having the models so closely at hand, took alarm and rejected similar sound-combinations.

LABORATORY TESTS OF ANGER, FEAR AND SEX INTEREST

By Henry T. Moore, Dartmouth College

The following experiments were devised with a view to determining approximately by a simple behavioristic test an individual's liability to be disturbed by emotional stimuli of certain definite kinds. Stated briefly, the plan of procedure was to measure the effectiveness of emotional disturbance in terms of delay in the solution of a problem given just prior to the introduction of the emotional stimulus.

The problems given were mental multiplications of selected numbers between 64 and 99 by 4, 6, 7, 8 and 9. The various number combinations were made as nearly as possible of the same difficulty, and proved to be of a sort that required from five to fifteen seconds for the average student to solve correctly when left undisturbed. The average of the mean variations of fifteen subjects for four problems each under normal conditions was 4.2 sec., hence this kind of task seemed to be one in which variations of more than six or seven seconds in the time of solution were likely to indicate something more than the varying difficulty of the task itself. Whenever a considerable delay resulted from the application of a distracting stimulus, the amount of this delay would seem to be in some sense a measure of the amount of distraction. Since, however, the special concern was not with degrees of distraction, but with amounts of anger, fear, etc., it was necessary to eliminate as far as possible the effect of general distractibility in each case of emotional stimulus. For example, in attempting to determine the extent to which an individual was annoyed by an anger provoking stimulus, it was not possible to compare his time under provocation with his normal time, for the reason that he might be easily distracted by any kind of irrelevant stimulus, and yet not very irascible. To get at the specific anger effect it was necessary to compare his "anger time" with the average of all the "distraction times" taken together.

Twenty-two subjects were given twenty problems, during each of which some sort of emotional distraction was at-

tempted. The twenty problems were divided into five series of four problems each. The stimuli accompanying the first series were intended to provoke anger; those with the second series to provoke fear; with the third, sex interest; with the fourth, repulsion; with the fifth embarrassment in the presence of a crowd.

Anger Stimuli. These tests were meant to involve (1) anger at an unjust accusation, (2) anger at bodily annoyance at the hands of another person, (3) anger at having been prevented from carrying out the task called for.

The attempt to excite anger at an unjust accusation was made as follows. Before the subject was aware that the experiment proper had begun, the experimenter stopped and said,—" By the way, Mr. S. Before I forget about it, I think I ought to bring to your attention a letter which came to me to-day, and which I am sure contains an error on someone's part that you can set me right about." The experimenter then handed him a typewritten letter purporting to come from the offices of administration, and which read as follows: "I have just learned from the registrar that an examination proctor has filed a statement in which he expresses it as his opinion that Mr. S. was guilty of dishonesty in connection with one of the mid-year examinations. While the circumstances do not amount to proof, it is advised that all those instructors with whom Mr. S. is taking courses give especial attention to his methods of class-room work, with a view to determining what measures should be adopted in dealing with his case." After having allowed fifteen seconds for the reading of the letter, and having remarked that it might be better to leave the consideration of it until later, as someone had undoubtedly made a mistake, the subject was asked to solve the first problem. The difficulty with this particular stimulus is that not all of the subjects lend the same degree of credence to the letter.

Two tests of anger at bodily annoyance at the hands of another person were next in order. During the solution of one problem the experimenter had a third person draw his hand sharply upwards across the face of the subject, striking smartly the tip of his nose in passing. A similar test consisted in striking the subject sharply three times on the cheek during his effort to solve the problem.

A third type of provocation was that of having an assistant interfere in three successive efforts made by the subject. The interference consisted in saying aloud a series of numbers calculated to make it impossible for the subject to carry out

successfully his mental multiplication. After three such disturbances he was allowed to finish without interruption.

Fear Stimuli. Four types of fear stimulus were tried. They were devised with a view to bringing into play fear of snakes, of personal attack in the dark, of electrical shock, and of falling.

As a test of the disturbing effect of the fear of snakes, the subject was seated in the center of a room completely dark, and some reference was made to snakes. Immediately thereafter the problem was given him, and while he was attempting the solution, a five foot length of rubber tubing was drawn slowly around his neck, coming in contact with the skin at the back.

During the solution of a second problem the fear of personal attack was brought into play by having the assistant, who had all the while been concealed noiselessly behind the chair in which the subject was seated, suddenly place his hand, slightly chilled from recent immersion in cold water, about the forehead of the subject.

For the third fear test the subject was seated on the threshold of a door which opened above an eighteen inch drop into an adjacent room. After allowing him to become aware of the distance to the floor behind him, his chair was tilted back at such an angle that he was to fall back into the next room as soon as released. He was then asked to close his eyes, and the assistant noiselessly took a position behind the chair and made ready to catch it at a distance of about one foot from the floor. Immediately after beginning the solution of the problem the subject was allowed to fall almost to the floor; he was then lifted to the original tilted position and held there until he succeeded at his task.

During a fourth problem the disturbing influence was the expectation of receiving an electrical shock of unknown intensity. After professing to him that the test was to be one of ability to endure pain, and asking him if he knew how many volts he could endure, the experimenter had him take in hand two exposed electric wires which seemed to be connected with a wooden box containing four electric lamps, which lighted up every time the experimenter turned on the current by snapping the switch. Prior to taking the exposed wires the subject had seen the experimenter turn on the current twice as if to make sure that the connections were all intact. Upon giving out the problem to the subject the experimenter moved toward the switch and at the end of five seconds turned it on; immediately thereafter he turned it off, and

began to busy himself pretending to rectify an apparent break in the connection of the wiring, such as might satisfactorily explain to the subject why he had felt no shock when the current was first turned on. After ten seconds of such delay the switch was again snapped on, and the same pretence gone through with as many as three times, unless the subject had meantime solved the problem.

Sex Interest. The four stimuli used for this instinct were all of the same sort, for the reason that only one kind of stimulus suggested itself as being both practicable and likely to produce some degree of excitement. Four photographs of well-known nudes,—" La Source " by Greuze, two paintings of Venus by Titian, and a Venus by Giorgione answered the purpose. The subject was asked to perform his multiplication while looking at a nude.

Repulsion. Of the four stimuli used, one was smell, one visual, and two involved a combination of smell and touch. First the subject submitted to a moderately strong whiff of asafoetida; next he multiplied while looking at a picture of human entrails; his third problem was done while holding a human brain over a jar in which the odor of formalin was quite distinct, and the fourth while he immersed his hand in the midst of a number of sheeps' brains in another jar which also contained formalin.

Embarrassment. In order to test the degree to which the subject's mental operations would be retarded by his having to face a crowd, he was given four problems to solve while seated conspicuously in the presence of a class room full of watching students. In most cases the number of onlookers was sixty-five, but for a few men it was necessary to use a group of twenty-three.

After taking an average of the subject's times for the twenty problems, all of which were given with some accompanying distraction, a separate average was made of the times of the anger series, fear series, etc. When the anger time was then compared with the general average time, an anger score was computed in terms of the percentage of increase or decrease over the general average time. When the percentage of increase was great, it was interpreted as meaning that the element of anger, over and above general distraction, was peculiarly effective in retarding that individual's reactions. By comparing the various percentages of the different subjects it was now possible to rank each of the twenty-two subjects with respect to each of the five emotions, rank 1 indicating the

greatest and rank 22 the least amount of disturbance for a given emotion. The table of percentages and ranks follows:

Subject	Anger %	Anger Rank	Fear %	Fear Rank	Sex interest %	Sex interest Rank	Repulsion %	Repulsion Rank	Embarrassment %	Embarrassment Rank
Bo...	80	2.5	2	17	-28	17.5	-34	15.5	-24	12
Br...	-5	12	3	16	37	1	3	7.5	-39	17
Bw..	-29	18	120	1	-84	22	-35	17	27	4.5
Ca...	2	9	11	15	-28	17.5	20	10	-4	10
Cl...	-43	19	23	10.5	-25	16	-81	22	148	1
Cr...	-55	21	23	10.5	9	9	5	5.5	17	6
Do...	4	8	77	3	19	4	-37	18	-35	14
Fe...	31	6	-39	22	14	14	-6	9	X	X
Gr...	29	7	-20	20	-39	19	3	7.5	27	4.5
Ho ..	-48	20	56	6	-13	13	-31	14	34	2
Li. ..	108	1	39	7	-65	21	-24	13	-59	19
Ly...	80	2.5	-7	18	-60	20	-42	20	28	3
Mu..	-4	10.5	30	9	-3	11	-21	11	X	X
Ni...	-21	16	58	5	-9	12	-34	15.5	6	9
Pa...	-6	14	12	14	12	8	5	5.5	15	7
Pe...	-28	17	76	4	-24	14	-39	19	14	8
Qu ..	-4	10.5	19	13	14	5.5	14	3	-60	20
Sm..	48	4	22	12	13	7	-55	21	-28	13
St. ..	-57	22	102	2	4	10	9	4	-57	18
Wh..	-6	14	-25	21	-25	16	66	1	-7	11
Wr ..	-6	14	31	8	31	2	-22	12	-36	15.5
Wt ..	37	5	-10	19	27	3	-20	10	-36	15.5

As a partial check on the above rankings sixteen of the subjects were asked to rank themselves on a scale of twenty-two places, each judging by what he knew of his own emotional traits and those of his fellows. Sixty-two statements were obtained from the sixteen men. It was found that the average deviation of a subject's estimate of himself from the ranking of the test was 4.5 places on a scale of twenty-two places, which is equivalent to a positive correlation of .37. For fifty-seven of the sixty-two judgments the average deviation from the test ranks was only 3.5 places, or the equivalent of a correlation of .51; and twenty-six of the judgments fell within one place of the corresponding test rank. A further and more complete check, which is to be applied as soon as the data are available, is the correction of the test ranks with an order of ranking based on the consensus of opinion of the whole group, each subject judging every other subject with respect to the five emotional traits.

Conclusions

1. The individual variations in the above test are quite sufficient to make it possible to rank a group of subjects in

respect to the amount of interference created by a given emotion.

2. Individual differences are greatest in respect to the capacity for anger and embarrassment, which show a mean variation of 35 on the scale of percentages used; sex interest and repulsion are more constant from individual to individual, as indicated by mean variations of 27 and 24 respectively. Fear stands midway with a mean variation of 31.

3. Fear caused by far the most powerful disturbances in the thought processes involved. The average fear time was 28 per cent higher than the general average of all the emotion times. Next followed anger with an average of 4.86 per cent more than the general average; embarrassment ranked third with minus 3.5; sex interest fourth with minus 10; and repulsion fifth with minus 16.

4. A comparison of the results for the different emotions suggests that an individual's capacity for fear and for anger are to a certain extent mutually limiting quantities. Their negative correlation of minus .48 is large enough to be considered significant. None of the other correlations were large enough to call for especial comment.

The writer is at present engaged in extending these tests to a larger number of subjects. A beginning was made with 125 individuals, but war conditions have since reduced the number to 44. Two more stimuli have been added to the list for fear and anger, and as a further check on the validity of the method, its results are to be compared with those from a series of memory tests in which the chief point of memorability is to be the emotional interest of the material. Before the method can be entirely satisfactory it will be necessary to arrive at a set of stimuli all of which are accepted at face value by the subject, as were the falling and class-room tests in the above experiments. It is evident also that the variety of stimuli for each emotion should be large enough to cover a considerable number of typical cases.

RETENTION OF SKILL AFTER LAPSE OF PRACTICE: SIMULTANEOUS READING AND WRITING

By June E. Downey and John E. Anderson, University of Wyoming

In the twenty-sixth volume of this Journal we reported an experiment upon Automatic Writing, the outcome of which was of interest chiefly because of its bearing upon the possibility of maintaining two simultaneous activities, in this instance reading and writing. As it required considerable effort and practice in order to acquire such measure of skill as we did attain in manipulating the double process, it seemed to us when opportunity offered worth while testing the retention of capacity after a long interval of non-practice. Our last experimental session in any series of the above investigation took place May 14, 1914; our first in 1916 on July 28. In the 1916 investigation the conditions of 1913-14 were duplicated exactly. Both subjects (*D* and *A*) were, however, in better physical condition in 1916 than in 1914; *D* decidedly so. The series of 1916 were carried on solely for purpose of comparison of expertness after the long lapse in practice and were confined to reproduction of only two of the situations dealt with previously, namely writing a memorized verse, (I) while maintaining silent reading, and (II) while reading aloud. In every case the writing was screened from the writer. In 1914, we had found that after much practice we succeeded in bringing the simultaneous processes somewhat closely to the normal speed-limits but the situation was so complicated and required such tension of effort, that we would not have been surprised to find that our hard-won virtuosity had wholly evaporated. This did not prove to be the case.

For directly testing retention of capacity our records, however, leave much to be desired. Our purpose in 1913-1914 being primarily to study the conditions under which automatic writing appears, we varied certain factors in a way that introduces some range of error in the determination of practice effects. For example, the length of the writing interval was governed, in part, by the natural divisions of the story we chanced to be reading. This allowed us to test the effect upon

'distraction of attention' of an increased interest in the story as it approached a climax. In part, we deliberately shortened or lengthened the writing period in order to note the effect of warming-up and of fatigue upon automatic writing. For our present purpose a constant time-interval or a constant number of times of writing the memorized verse between pauses would have been more satisfactory. However, the records make possible some very interesting comparisons.

Before reporting the facts with respect to the double process we shall make a statement concerning the normal spurted speed records of 1913-1914 and 1916, for both reading and writing. The writing records are in terms of the time in seconds required for writing the test verse; the reading-rate in terms of the number of words read per second. In the interval between the practice at speeding, both writing and reading had, of course, continued as normal activities. *A,* as a university graduate student, has presumably practised spurted writing in connection with the taking of lecture notes; *D* had, on the contrary, become interested in a printing movement in writing which might be expected to cause some retardation in speed. So far as reading was concerned, a retardation in *A*'s rate might reasonably have been anticipated from the fact that he had done in the interval considerable critical reading of philosophical prose. *D,* on the other hand, had made some conscious effort to increase her reading rate.

In getting our normal writing-speed for the given verse, perfectly memorized, we had found in 1913-1914 a progressive increase in speed with, as we had thought, an approach to a physical limit in the case of *A,* with whom the investigation was carried out at much greater length than with *D*. The normals for the writing-rate had been obtained both with and without the use of a screen, the records with the screen being consistently more rapid than those without it. *A*'s gain on his initial record, 1913-1914, without the screen, was 5.2 seconds or 12.3 per cent of the initial record. We did not begin taking the normal without the screen until the experiment had run some time so that our records in this respect are inadequate. Nor have we a sufficient number of normals with *D* to make a comparison of much value.

It is, however, possible to compare in *A*'s case the average, the fastest, and the slowest records of the last seven trials in 1913-1914 with the average, the fastest, and the slowest of the first seven trials of 1916. These results are presented in Table I. The last speed records of 1913-1914 were taken for *A* in February, 1914; for *D,* in November, 1913.

TABLE I

NORMAL SPEED RECORDS. WRITING TEST VERSE

Reagent	With Screen								Without Screen							
	1914				1916				1914				1916			
	Av. last 7 trials	M.V.	Fastest	Slowest	Av., 7 trials	M.V.	Fastest	Slowest	Av., last 7 trials	M.V.	Fastest	Slowest	Av., 7 trials	M.V.	Fastest	Slowest
A	35.7	1.2	33.0	38.0	33.3	1.6	29.8	36.0	36.9	1.1	33.5	40.2	37.5	1.6	34.0	42.0
D					*57.0	2.48	52.5	60.0	†58.94	2.58	54.4	63.2	57.3	3.96	52.0	63.0

* Average of four trials only. † Average of five trials only.

In the case of *A* the average of the first seven trials with the screen is in July 1916, 2.4 seconds or 6.7 per cent more rapid than the last seven in 1914. The fastest speed record for 1916 is 3.2 seconds faster than the speediest for 1914. Without the screen the average for the last seven 1914 is 0.6 seconds faster than the average for 1916, less, however, than the M. V. In both instances, there is a slight increase in the M. V. for 1916.

We interpret this to mean that the effort at increased speeding was checked by the visual perception of the result when the screen was not used; and that, in 1914, *A* had practically reached the limit of writing rapidly consistent with such measure of visual conscience (for penmanship!) as he possessed, advancing from over three to more than four letters per second.

A comparison of the illegibility of the records confirms this conclusion. All of *A*'s spurted speed records are highly illegible but those with the screen more so than those without. A comparison of the first record with the screen, 1916, shows only one second's difference from the last of 1914, a loss less than the mean variation on the last series. The practice effect continues from the first trial on, when the writing is screened from view, although spurting is carried no further in 1916 when writing is visible.

For *D*, who wrote the verse for only one series of experiments in 1913-1914, there are only five records without the screen; the average, 58.9, shows slightly greater rapidity than the average for five in 1916; the M. V. in 1916 is, higher. There are no adequate records for a comparison of the normals without the screen.

We may next consider the normal records for rate of reading in the different years, Table II. These records afford less opportunity for exact comparison than the writing records since they are, in part, dependent upon the text read, upon the stylistic qualities of the author, and upon the variation in interest in the story from point to point. There is perhaps some evidence of a slight increase in *D*'s silent reading-rate, 1916, and a slight decrease in that of *A*. Reasons for this have already been suggested. *A*'s rate of reading aloud would, however, seem slightly increased. The rate of reading aloud is greatly dependent upon degree of effort. In 1914, *A* had learned a means of controlling the voice, etc., that had progressively increased his rate but at a sacrifice of enunciation. It was the impression of the experimenter that in 1916 there was, very evidently, an increased distinctness of enunciation

TABLE II

NORMAL SPEED RECORDS. READING WORDS PER SECOND

Reagent	Silent					Aloud				
	1914			1916		1914			1916	
	First trial, Oct.	Av., last 5 trials, Nov.	Range through whole series	Av., 4-6 trials	Range	First trial	Av., last 7 trials, Apr.-May	Range, Dec.-May	Av., 7 trials	Range
A....................	8.9	10.4	8.6-12.5	8.2	7.3-9.0	3.33	4.88	3.33-5.15	4.24	3.26-5.67
D....................	7.4	6.7	5.4-7.6	8.0	6.5-9.2	4.03	5.01	4.01-5.76	(one trial) 3.98	....

TABLE III

SPEED RECORDS. WRITING TEST-VERSE. DISTRACTION

	Reagent	1913-1914					1916					1914-1916	
		Av., first lap	Av., last lap	Whole No. of trials	Absolute gain	Relative gain, per cent	Av., first lap	Av., last lap	Whole No. of trials	Absolute gain	Relative gain, per cent	Absolute loss, 1914-1916	Relative loss, 1914-1916
Writing Distraction I	*A*	57.58 M.V. 2.16	42.2 M.V. 1.07	116	15.38	26.0	44.1 M.V. 2.05	36.7 M.V. 2.5	35	7.4	16.8	1.9	4.5
	D	89.38 M.V. 13.8	64.27 M.V. 1.48	232	25.11	28.1	71.5 M.V. 5.4	59.7 M.V. 1.9	37	11.8	16.5	7.23	11.2
Writing Distraction II	*A*	71.21 M.V. 15.3	37.96 M.V. 3.01	168	33.25	46.7	62 M.V. 3	37.6 M.V. 1.7	43	24.4	39.4	24.04	63.3
Silent Reading	*A*	7.7	9.3	10	1.6	20.8	6.1	8.01	5	1.91	31.3	3.2	34.4
	D	2.4	4.2	11	1.8	75.0	?	4.86	4	?	?	?	?
Reading Aloud	*A*	2.31	3.62	23	1.31	56.7	2.5	3.52	18	1.02	40.7	1.12	30.9

in conjunction with rapidity, a point of some interest and possibly dependent upon general developmental factors.

Let us turn now to the records when reading and writing are run together, Table III. In our first series of experiments writing was maintained in connection with silent reading (Distraction I). In this instance we are able to make some interesting comparisons, first, between the last lap in 1913-1914 written in pencil, and the first in 1916. The last series of all in 1913 is not taken into consideration inasmuch as this record, contrary to the usual custom, was done in ink. The elimination of this series introduces, however, no error since the records of this day are poorer than those of the preceding session, except in two instances for *D*.

A's first lap (average of 11 trials) in 1916 shows on the face of it a loss of only 1.9 seconds from the last lap of Nov. 1913 (10 trials), a variation which is only slightly more than the mean variation of the last lap of 1913. It must, however, be remembered that *A*'s practice in writing the given verse under distraction was continued with reading aloud after the distraction of silent reading was dropped, so that it would be more significant to compare his final record under distraction (II) for 1914 with the first for 1916. This final record was 37.96 and using this as a basis for comparison we find a loss of 6.14 seconds or 16.1 per cent. As reading aloud was decidedly more distracting for *A* than was silent reading, it seems probable that this loss of more than 6 seconds lies within the actual loss from lapse of practice. *D*'s loss on the final record was 7.23 or 11.2 per cent. Both *A*'s and *D*'s records show in 1916 an increase in the variation of the individual speed records from the average.

Reference to the records shows that by the third day's practice (1916), with silent reading as a distraction, both *A* and *D* had passed beyond the average record of the last lap for 1913-1914 and had established a new speed record. The question arises whether there has been an actual gain in the interval of non-practice in the speed with which learning went on. Our general impression from the introspective ease with which the process was maintained was that this was true. Later, we shall need to reconsider this conclusion.

The experiment involving writing while reading aloud (Distraction II) was carried out in 1914 with *A* alone. Here the loss from the final lap of 1914 to the initial one of 1916 was 24 sec. or 63 per cent. These records are not subject to the error of Distraction I. A comparison of the two records

shows in any case a decidedly greater loss for the situation which for *A* was the more complicated one.

Turning now to the reading records we find that *A* has lost considerably in rate of reading. It is, however, impossible to determine how much this is due to loss of capacity for carrying on the double process since there has also been a decrease in the normal rate of silent reading. It is probably better to confine ourselves to the writing records.

The question raised above then becomes paramount. Since the learning process is one in which a practice effect is continuous, does the rate at which the practice effect goes on increase, decrease, or remain constant after the long interval of rest? There is, possibly, some difference in the case of the two subjects.

In 1916, *D* wrote the verse thirty-seven times under distraction of silent reading. Her initial record was 83 seconds; her final record, 56 seconds; a gain of 27 seconds or 32.5 per cent in the thirty-seven trials. If we take the last thirty-seven records of 1913-1914 (ink records omitted) we find a drop from 70 seconds to 62.4 seconds or a gain of 7.6 seconds, that is, a gain of 10.8 per cent in 37 trials. The rate at which practice went on would seem then to be slightly increased in 1916. Recovery of skill in 1916 took place rapidly so that by the fifth trial *D* is within 0.8 seconds of her final average for 1913. From that point on until the close there was a gain of 13.8 per cent.

If we compare the averages of the laps instead of individual records, we find a confirmation of our conclusion as to an increased rate of improvement. On Nov. 11, 1913, *D*'s average for sixteen trials was 71.52; in July, 1916, the average of thirteen trials (first lap) was 71.50; practically the same. But in the two following laps of approximately the same number of trials there was a fall to 64.27 in 1913 or a gain of 10.1 per cent; in 1916, to 59.7, a gain of 16.5 per cent. There is, then a slight increase in the rate at which practice went on for D. The M. V. is, however, slightly higher in 1916.

A's record, Distraction I, is subject to less obvious interpretation because of the complication of the practice effect with that of Distraction II. He did not, however, approximate his average final record on verse-rapidity with Distraction I, 1913, until his eleventh trial in 1916; nor his final average for verse-rapidity, 1914 (if we take both forms of distraction), until his fifteenth trial. From that point on his relative rate of increase, 1916, was 12.7 per cent, very similar

to *D*'s rate of increase. In 1914, the last twenty trials show no constant increase so that we had concluded that *A* had reached his practice limit.

In 1916, *A* wrote the verse, while reading aloud, forty-three times. The initial record was 60 seconds; the final record, 34.2 seconds, a gain of 25.8 seconds or 43 per cent. If we take the last forty-three records of 1914 (ink records omitted) we find a drop from 49.3 to 36.9, or an increase in rapidity equivalent to 25.1 per cent. Approximation to the final *average* record of 1914 did not occur, until the thirty-ninth trial in 1916; and rapidity increased in the next five trials only 10.5 per cent. The practice effect manifests itself here in a very rapid re-learning. If we take the averages of the laps we find that on Feb. 19, 1914, (44 trials before the close) *A*'s average for six trials was 47.95. In July, 1916, the average of his first four trials was 62. In 1914, the record fell, in forty-four trials, to 37.96—a gain of ten seconds or 20.9 per cent. In 1916 there was a drop in forty-three trials, from 62 seconds to 37.6 seconds, or 39.3 per cent gain. But in large measure the process is one of re-learning.

Unfortunately we do not possess enough data to be able to draw conclusions as to the difference between *D*'s record with distraction I and *A*'s with distraction II. We expect, however, the greater loss to occur for the more complicated process. Possibly, too, the fact that *A* in 1914 approached more nearly the limit of his capacity for carrying the double process than did *D* accounts for some of the difference. Possibly *D*'s improved condition in 1916 is a factor in the case.

Table IV summarizes other data that bear on a comparison of the records of the two reagents. First of all, we note that the relative loss on the normal in the first four trials for 1913 was with Distraction I much higher for *D* than for *A;* in 1916, the relative loss has become practically the same for both.

In this connection we tested *D*'s ability to maintain writing while reading aloud. In 1914, *D* did not try this experiment. Our notes show that in the series in which mental arithmetic was used as a means of distraction, she was unable to carry the double process because of difficulty in handling mental arithmetic. No record occurs concerning writing while reading aloud. If our memory is correct, however, we found the series so long with *A* (twenty-three days of from one hour to two hours' practice each were required to approximate the normal average) that we did not have time to use both subjects. Judging from the ease with which *D* handled the

TABLE IV

SPEED RECORDS. WRITING TEST-VERSE AND NEW VERSE

Reagent	Year	First normal	Average, first four trials	Absolute loss	Relative loss, per cent	First normal	Average, first four trials	Absolute loss	Relative loss, per cent
		Distraction I				Distraction II			
		"Thirty days hath Sept."				"Thirty days hath Sept."			
A	1913-14	39	56.3	17.3	44.4	39	82.8	43.8	112.3
A	1916	34	44.6	10.6	31.2	36	62.0	26.0	72.2
D	1913	59	97.5	38.5	65.3	Not tried.			
D	1916	60	79.5	19.5	32.5	52.5	79.3	26.8	51.0
		"Mary has a little lamb."				"Mary has a little lamb."			
A	1916	23	26.6	3.6	15.7	23.5	43.5	20.0	85.1
D	1916	32	34.0	2.0	6.2	31	50.5	19.0	61.3

given verses in 1916, she would have found this test relatively easier than distraction I. In our notes on taking dictation while reading aloud we noted in 1914 that, relatively, *D* read aloud much more rapidly than she read silently, a fact true also for her normal reading rate.

With Distraction II, *D*'s first attempt in 1916 shows less loss on her normal than does *A*'s first trial in 1916 after 168 trials in 1914. Her relative loss is less than for the average of her first four trials with Distraction I. She had a feeling of having carried over to the new situation the skill acquired in Distraction I; if so, she transferred this capacity very much more effectively than *A* gave any evidence of doing.

In this particular experiment we have, on the one hand, a motor factor involving the unwinding of a series of mechanical movements corresponding to the verse words. On the other hand, we have the maintenance of a double set (attentional). The difference between *A*'s and *D*'s records can be understood if *A*'s acquirement of skill is primarily motor, an unwinding of specific movements, and *D*'s due to a general set of attention, which might more easily function in a modified situation.

In order to obtain data for determining to what degree success in writing the given verse was due to a special motor habit and how far dependent upon general habits of attention we shifted finally to another verse and wrote it while maintaining (I) silent reading and (II) reading aloud. Table IV presents the results.

In *D*'s case, the second verse with silent reading as the distracting process, goes easily from the first. Apparently the whole effect of practise is carried over. *D* reported, however, that the second verse was very much easier to handle than the first, because of its employment of simpler words and a more regular rhythm.

Likewise in *A*'s case the writing of the second verse during silent reading goes easily from the first, indicating that a considerable practice effect is carried over to the new verse. In *A*'s case, however, the relative loss, while only a third of that for the original loss on the test verse in 1913-14, is still twice as great as that of *D* on the new verse in 1916 (which was one-tenth of that on the test verse in 1913). The relative loss of *A* on the new verse is one-half that on the repetition of the test verse in 1916, while that of *D* on the new verse is one-fifth of her loss on the repetition of the test verse in 1916. This confirms the existence of a difference in *A*'s and *D*'s type of reaction to the general situation.

With distraction II, *D*'s relative loss on the new verse is somewhat greater than that on the test-verse, but the latter had the benefit of the practice effect of writing under distraction I. *A*'s record shows some evidence of transfer effect, if we go back to the initial record in 1913; but there is evidence of loss if we consider only the final record. In any case, D handled the situation more easily than A, in spite of his long practice. We conclude that the two subjects give evidence of a significant difference in reaction.

Introspectively *A* reported that in the first fourteen trials 1916 (Distraction I) he went through the whole course of development of 1913-1914. The writing-cue became progressively bigger and bigger. In some respects it seemed to *A* that writing was more automatic than it had been two years before; there was a tendency more frequently to lose his place so that he was bothered by the feeling of not knowing where he was at in the verse.

Lapses were exceedingly frequent in *A*'s 1916 records. In some cases these lapses are simply the cutting of strokes and the dropping out of letters so evidently the result of spurting. The limit toward which *A* was obviously moving was a mere scratching of the pencil, wholly indecipherable, synchronous with the feeling of line-meaning. Before this point was reached in 1916 it was possible to observe some of the old lapses reappearing. Others wholly new put in an appearance, for instance, a confusion in the order of lines so that all are present but in a curiously mixed order. Another change in the appearance of the writing was a shift in the length of the verse lines. *A* broke away from the conventional arrangement in such a fashion as to keep the length of the lines uniform. The intention to spurt operated and induced new methods of increasing speed. These shifts in reaction indicate a relatively greater flexibility in handling the material in 1916 than at the close of 1914 and explain the possibility of *A*'s passing beyond the record of 1914 in which practice had apparently reached its limit. The rest-interval, with its breakup of certain mechanisms, gave freedom for new adaptations. Book expresses it from the other side when he states that the rest-interval operates in causing the dropping out of bad habits.

As was true in 1913-1914, *D*'s lapses in 1916 are in a large measure conspicuous by their absence. Where they appear they are the old lapses of 1913, namely a doubling of the initial stroke on the "y" and "n" and a repeating of "i," with the old curious loss of all motor report for this tiny stroke.

Summary. There is considerable retention of capacity to maintain two processes (reading and writing) after lapse of practice for more than two years, with a rapid re-learning and approximation of one's last record. Subject *D* showed a quicker recovery than Subject *A*. The records of *A* also indicate a much greater loss of capacity for handling the situation that he found the more difficult of the two utilized.

Transfer of the practice effect appeared more evidently for subject *D* than for subject *A*. Since the situation involves both motor and attentional factors, it suggests itself as an excellent one to use in a specific investigation on generalized habits.

A LIMEN COLOR MIXER

By A. P. Weiss, Ohio State University

1. General Description.
2. Differential Color Discs.
3. Use of Mixer for Research.
4. Use of Mixer for Demonstration.
5. Description of Details.

1. General Description

The limen color mixer described in this article was designed to permit fine variations in the percentages of the color components entering into a rotary mixture of colored papers or discs, and to allow accurate control of the duration of the exposure of the colors and the intervals between the exposures. The problem for which the mixer was originally designed was to determine the effect of the adaptation of the eyes to various colors, upon the recognition or discrimination of colors.[1]

To illustrate: Suppose it is desired to determine whether green is more easily recognized as green when the eyes have been first stimulated with red, than when they have not been stimulated by any color other than the normal illumination. To answer this question it is first necessary to determine for the normal unadapted eye what percentage of green it is necessary to add to (say) a black in order that a definite greenish hue may be detected in the black. With a series of differential discs such as those described under 2, it is possible to begin with black only and gradually add small increments of green until the observer is definitely able to detect the green component in the black.

The next step is to stimulate the eye by fixating a large sheet of red paper for a definite period and then determine whether the percentage of green which must be added to the black to make it appear greenish is now greater or less than for the normal condition. As will readily be seen this experiment may be modified in many different ways. Industrially,

[1] The apparatus was used for this purpose by Miss Mary Almack, graduate student in psychology. *Jour. of Exp. Psych.*, Vol. I, No. 5, 1916, p. 426.

it may be used to determine the effect of the various types of illumination on the recognition of colors. The apparatus is relatively simple and quickly manipulated so that unpractised observers may be used.

The limen color mixer may be regarded as a combination of four color mixers, any one of which may be shifted to such a position that only a small area of the rotating disc is exposed. The position of the exposed area can be accurately determined by a micrometer screw arrangement; and from these readings the percentages of the color components may be derived. In general the apparatus may be regarded as a device which permits the control of the following conditions:

1. Many different color combinations are promptly available.
2. Only a small area of the color to be judged is exposed.
3. The percentage composition of the exposed color can be accurately determined.
4. By means of differential discs the increments by which a given color or brightness is changed can be made as small as necessary to meet any condition.
5. The length of time that the color is exposed, and the duration of the interval between successive exposures, can be controlled.
6. The exposure shutters move from the center outward when opening, and in the opposite direction when closing, thus eliminating any tendency toward eye movement.

2. Differential Color Discs

The subliminal steps that are necessary for determining color limens or thresholds are secured by superimposing one color upon another in such geometrical proportions that at various distances from the center of the disc, different proportions of the two colors will occur. One of the simplest geometrical shapes that will permit this is illustrated in figure 1. A cardboard disc 10 inches in diameter is the most satisfactory. Larger discs are likely to be torn during rotation and in smaller discs the radii along which the series of combinations is seen, are too short.

Suppose it is desired to pass from black to green by subliminal steps. If a green disc (fig. 1) has pasted on the face of it two circular black discs b, the part marked a will be green, and the part marked b will be black. When this compound or differential disc is then rotated in a color mixer, the resultant colors will vary from black only at the center, to green only at the periphery. Between the center and the

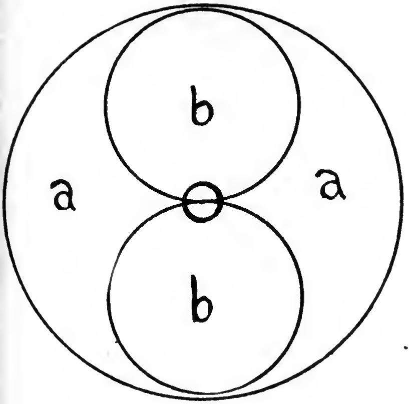

FIG. 1

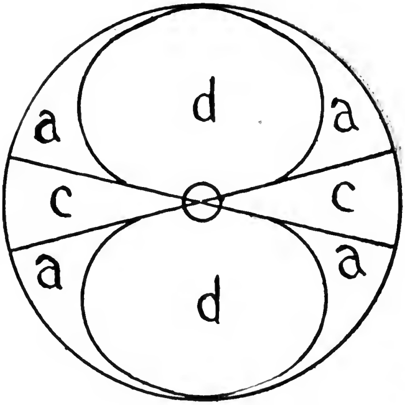

FIG. 2

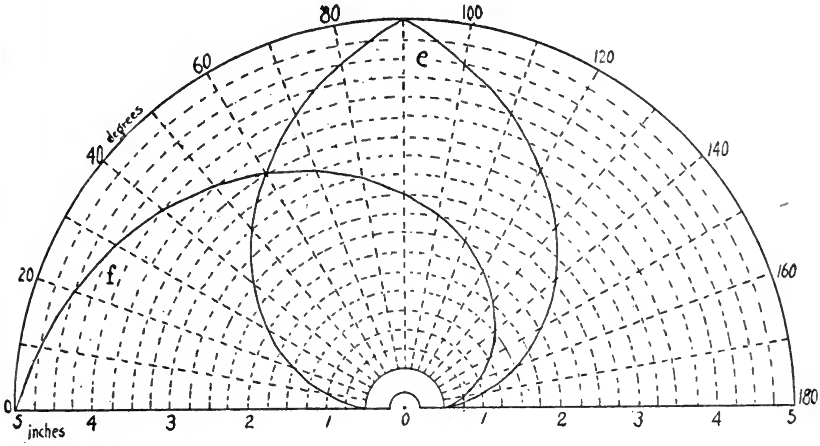

FIG. 3

periphery will be found all possible combinations between black and green. That is, if we consider one of the radii of the disc while it is being rotated, the face of the disc along this radius will not be all of the same color but will be a blended series of blacks and greens beginning with black at the center and becoming more greenish as the periphery is approached, until we have the green of the original color disc.

Referring this combination to the color pyramid we may say that this disc shows all the saturations and brightnesses

of the hue green, between the points black and green on the pyramid. If the circles (b) are a gray of the same brightness as the green, the differential disc will show all the possible saturations of green at the brightness of the original green disc. In this case the series will represent a change in saturation only. By selecting different hues and brightnesses of a and b all the colors between any two points on the pyramid may be shown as a continuous series. By having on hand fifteen or twenty of the various combinations, the demonstration of the color pyramid is much simplified.

Where it is desired to make the steps still more gradual than is indicated in figure 1, a pair of sectors (c) figure 2, of the same color as b may be added. These sectors increase the effect of the b circles by a constant amount, depending on the angular size of the sectors. They may be used also to change the hue of the resultant color. Thus if a is blue, b is red, and c is green, the resultant color on rotation will show combinations of blue, red and green.

In figure 1 the area of the two b circles is equal to the remaining area of the a part of the disc and under these conditions satisfactory series are secured only when the two colors are approximately equal in brightness. Where the differences in brightness between the colors which are to be combined into a continuous series is great, the darker color should cover a larger area than the lighter color. Thus figure 2 represents a combination in which a is light and b dark. Where the difference is very great, as between white and black, it may be necessary to add sectors c of the same color as d. For qualitative work such as class demonstration, b need not be a true circle; but for quantitative work it is simpler, from the geometrical standpoint, to make b true circles, and make whatever changes are necessary by adding the sectors c as was done in the discs shown in the illustrations of the limen color mixer, figures 4 and 5. However, when b is a true circle, the steps from the center outward do not represent equal increments of the color a. The series is geometrical in that the a color is added at first gradually and then more and more rapidly.

To secure equal steps in the addition of the a color with equal steps along the radius, the b color should have the shape of either e or f figure 3. In these shapes equal radial increments are plotted against equal angular increments on polar coördinates. Only one half of the differential disc is shown. The other half is, of course, symmetrical. Where there is difficulty in securing a high speed of rotation it is

best to use the shape e; where the speed is high, the shape f may be used. Both e and f are not, however, to be used on the same disc. If shape f is cut out of colored cardboard or out of another 10 inch disc, it may be used with other circular discs and thus extend its range of usefulness. It is not advisable to try to use shape e independently because it does not fasten so securely as f. These shapes are designed to allow for a thumb nut one inch in diameter. The shapes of e and f are not easily drawn and at best they are only approximations to the true shapes which would be secured if a very large number of points were plotted. Even for quantitative work it was found simpler to use the true a circles of figure 1 and measure the proportions of the a and b colors for any given radius of the disc, with a protractor.

The differential discs described in this section may, of course, be used on any color mixer. They are independent of the limen color mixer.

3. The Limen Color Mixer for Research

When used for research the large sheet iron screen 1 (fig. 4) covers the entire front of the apparatus with the exception of the slot 2 which exposes a shield 4 in which there is a round window 5 that travels from the center to the periphery of whatever disc is in the position 3. The size of window 5 is 5-8 inch in diameter. That is, the only part of the color disc which shows to the observer is the small area which may be seen through the window 5. To illustrate: If a differential disc rotating in position 3, is made up of (a) green and (b) black (fig. 1) the window 5 will expose a small area that will seem to be colored equally all over. If the window is near the center, the color will be less green than when the window is toward the periphery. By means of the screw 6 (fig. 5) the carriage which carries the window 5 and the shutters 19 and 19a, may be shifted anywhere along the radius of the disc, thus securing the various combinations of black and green.

Under ordinary conditions only the disc in the position of the spindle 3 will rotate; any of the other three discs 14, 15, 16, can be shifted quickly to the "running" position by turning the spider 7. By means of this spider the experimenter has four discs or series always available yet not visible to the observer.

The color discs are fastened to the spindles by a thumb nut as in the ordinary color mixers. The position of the window is accurately determined by the screw 6 (fig. 5).

This screw has a pointer on the scale 8 which indicates the number of turns of the screw. At 9 is a circular scale divided into one hundred parts. This is also provided with a pointer. The screw has eight turns per inch and the window 5 is thus moved one eighth of an inch along the radius of the disc for each complete turn of the screw. Since the circular scale 9 is divided into one hundred parts the position of the window may be read to one one-hundreth of an eighth of an inch or .00125 inch (.0318 mm). Assuming that the effective scale is four inches long, this will give a possibility of 3,000 objective steps between the center and the periphery of the disc, each step of which can be secured as often and as accurately as necessary.

This is of course much finer than is necessary for most quantitative work; but in designing the apparatus it was thought best to make the objective steps very small so as to be well within any discriminative limen which would likely be found. Where it is desirable to test the effects of various sources of illumination upon the recognition and discrimination of colors and especially where the changes in the illumination are subliminal, it is necessary to measure small increments in the objective colors very accurately.

The following are some of the research problems for which the apparatus is adapted:

1. Relative influence of hue, brightness and saturation upon each other as determined by discrimination.
2. Effect of various types of illumination (industrial) upon the discrimination or recognition of colors.
3. Effect of selected stimulation (adaptation) upon the discrimination or recognition of colors.
4. Determination of the color composition of after images.
5. Determination of the saturation and brightness limens for color combinations.
6. Effect of the duration of the stimulus and the interval between stimuli upon discrimination and recognition of colors.

4. The Limen Color Mixer for Demonstration

When the color mixer is to be used for demonstrational purposes the screen 1 (fig. 4) and the shield 4 are removed. If it is desired to rotate all four spindles at the same time this is done by passing a round leather belt over the pulleys of all four spindles. This method is used when it is desired to make direct comparisons between different color mixtures, say the different color combinations which will produce gray.

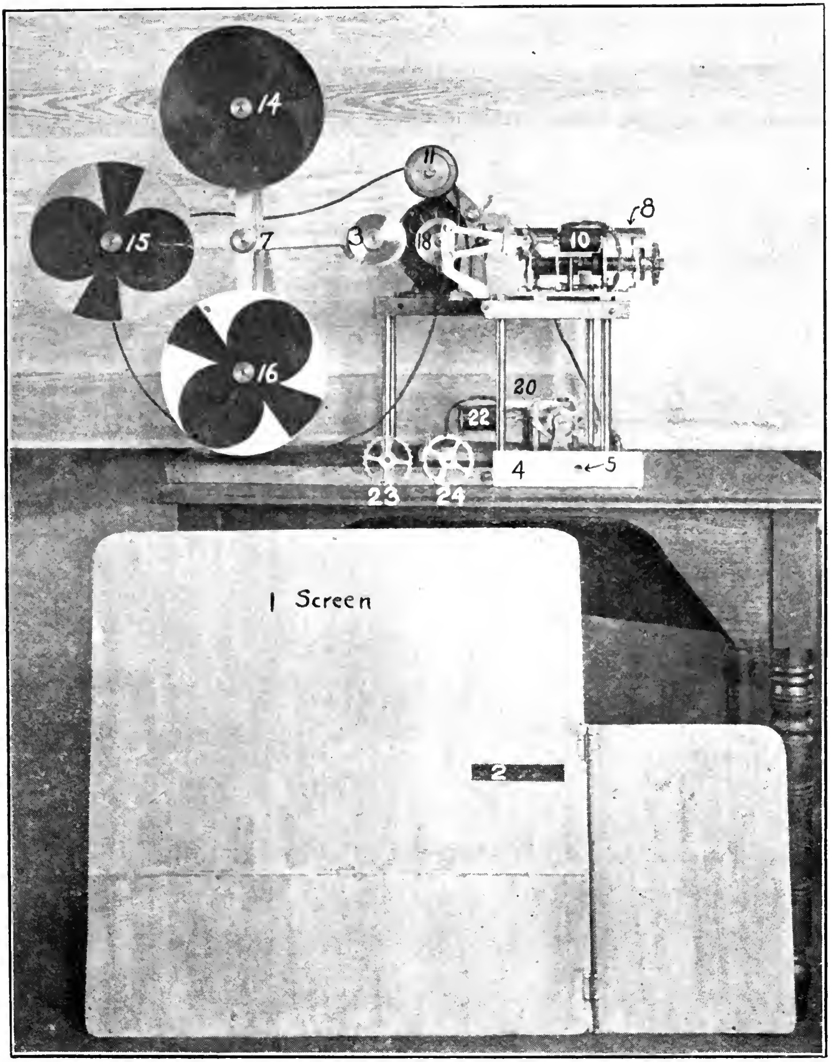

FIG. 4

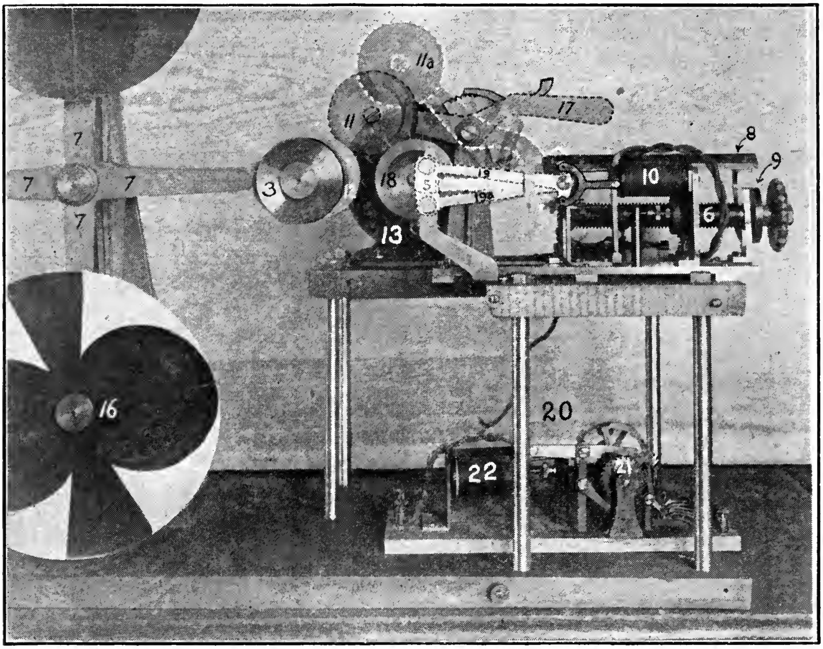

Fig. 5

For class work the limen color mixer lends itself well for the following:

1. Color mixing as it is usually carried out with the ordinary split color discs. Any sized disc up to 10 inches diameter may be used.

2. Continuous series by the aid of the differential discs described in section 2. Where the time is limited about twelve of the differential discs made up of the most important color and brightness combinations are sufficient. For more extensive demonstration, especially where it is desirable to show the manifold interrelations of brightness, saturation and hue combinations, thirty or forty of the differential discs may be profitably used.

3. By using 10 inch discs and having a light gray screen (wall or projection lantern screen) where the class can easily see it, one of the rotating discs may be fixated for a half minute and the after image projected on the screen. Such after images are unusually striking when differential discs are used.

5. Description of Details

The numbers which occur in the following description refer to figures 4 and 5 which are photographic reproductions of the limen color mixer.[2]

Figure 4 shows the apparatus placed on a table with the screen 1 removed and set up against the lower part of the table.

Figure 5 is a reproduction of a phantom photograph of the shutter 19, 19a and idler pulley 11 in both the "on" and "off" positions.

The figures by which the details are numbered refer either to figure 4 or 5.

1. Sheet iron screen, painted neutral gray, which covers the entire apparatus with the exception of window 5.

2. Slot in screen 1 through which window 5 is visible.

4. Tin shield attached to a carriage which is moved back and forth by the screw 6. This shield prevents the observer from seeing the color disc through the slot in screen 1.

5. Window, ⅝ inch diameter through which a cicular area is visible of whatever disc happens to be in the position 3. The distance of this window from the center of the disc is accurately determined by the screw 6. The opening and

[2] The apparatus was constructed by Mr. A. P. Freund, mechanician of the department of physics at the Ohio State University, from sketches furnished by the writer.

closing of this window is brought about by the aluminum shutters 19 and 19a which are operated by the magnet 10 attached to a carriage moved by the screw 6. The shutters open from the center outward and close from the periphery inward.

11. Idler wheel which acts as a friction wheel to turn spindle wheel 12; when in the position 11, the spindle will turn when the motor turns; when in position 11a, it is disengaged and allows the spider 7, which carries the other mixer spindles 14, 15, 16, to be brought into position 3 so that any one of them may be used. The idler 11 is moved by the handle 17 and can be shifted without stopping the motor 13.

13. One-tenth horse-power, alternating current motor, 3,200 revolutions per minute, which drives the color spindles by means of the grooved pulley 18.

20. Timing device modified after Kuhlmann. The ratchet 21 moves one notch whenever the magnet 22 is excited. By a suitable contact device and the proper contact wheels as 23 or 24, the window 5 can be kept open or closed for any period of time. To illustrate: Time wheel 23 has its contact teeth arranged in such a way that when the timer 20 is tripping once per second, the window 5 will give one instantaneous exposure per second. With wheel 24 the window will stay open two seconds and close one second (or the reverse). By selecting the proper wheels any relation of interval and exposure may be secured. The timer 20 is controlled by a pendulum or metronome which makes and breaks the electric current that excites the magnet 22. This pendulum is not shown because it is not an essential part of the apparatus. The pendulum which is used with the apparatus at Ohio State University is a duplex, self-acting electric pendulum designed by the writer. It has been used in many other experiments and has proved so reliable and satisfactory that its construction and method of operation will be the subject of a separate article.

THE FORMATION AND RETENTION OF ASSOCIATIONS AMONG THE INSANE

By Clark L. Hull

CONTENTS

I. Introduction

The experiments upon which the present article is based fall into two parts, those performed with insane subjects and those with normal subjects. The experiments with the insane were conducted at the State Hospital for the Insane, at Mendota, Wisconsin,[1] and those with normal subjects in the Psychological Laboratory of the University of Wisconsin.

The problem arose from a consideration of current practice respecting the methods of examining the memory of patients in insane hospitals. The patient, for example, will be told distinctly three or four facts such as the name of a color, a date, and a street address, with the warning that after a certain length of time, say a half hour, he will be asked to recall them. If at the end of that time little or nothing is recalled, and if in addition the patient is unable to recall certain of his more important recent experiences such as took place on his way to the hospital, his memory is considered "poor for recent events." Superficially, of course, such appears to be the case; and a diagnosis based upon such findings by men skilled in their interpretation may possess reliability. Nevertheless, it is at once apparent that under the expression "poor memory for recent events" as here used, are grouped without distinction, two things which are psychologically very

[1] The experiments at Mendota were conducted under the auspices of the Wisconsin Psychiatric Institute, directed by Dr. Wm. D. Lorenz. Dr. Lorenz has kindly verified the case histories and diagnoses included in this report. It is also with pleasure that the writer expresses his thanks to Dr. Frank I. Drake, superintendent of the hospital, and to the medical staff for their active cooperation while the experiments were in progress.

different, and which may quite probably be expected to vary more or less independently of one another. One is the rate of formation of associations, and the other is the degree of retention of these associations, once they are formed. For example in the case under consideration, a patient might fail in the recall either because (a) the name of the color, the date, and the street address made a very slight impression upon his nervous system—perhaps through poor attention, or because (b) though normally registered the impression was very rapidly obliterated, or (c) perhaps because of (a) and (b) both working together.

It was with a view to determining if possible, which if any of the above possibilities were actually taking place in each of three common types of mental defect, and the relative amount of disturbance in each, as indicated by the performance of normals under exactly similar conditions, that the present experiments were chiefly instituted. Incidentally, some interesting evidence was secured concerning fluctuations at the threshold of recall, which will be discussed in some detail in section V of this article.

II. Subjects

In all, nineteen subjects were used in the experiment, nine insane and ten normals. The latter were summer session students at the University of Wisconsin. The insane group consisted of three constitutional inferiors, three dementia praecox, and three paretics; they were very carefully selected from a group of over eighty patients who had previously been experimented on at some length, in an effort to adapt the Binet-Simon tests to the determination of the extent of intellectual degeneration among the insane. They were chosen on a basis of being tractable under experimental conditions, of being relatively clear types of their respective diseases, and of being at about the same intellectual level as shown by the very careful mental tests to which they had all been subjected. The intellectual level chosen was roughly, the ability to pass from thirty-five to forty of the Binet tests (Goddard's revision). Upon trial, however, it was found impossible for paretics of this grade of intelligence to learn the rather difficult material used, so a new group of paretics, of an intellectual ability sufficient to pass from forty-five to fifty tests[2] had to be substituted.

[2] Fifty-one was found to be the median number of tests passed by normal adults of about the same age and station in life. The so-called "wide-range" method of testing was employed.

The following brief case histories of the nine patients were compiled from the clinical records of the hospital, with special reference to memory:

Bur. Male, age 26. Constitutional inferiority with criminal episodes. He attended school for three years but learned little. After his fifteenth year he drank occasionally. He once threatened suicide to scare his father. He had stolen several bicycles and broken into a creamery. When admitted to the hospital he talked freely and relevantly regarding his misdeeds. Later he ran away from the hospital several times. Upon one occasion having been arrested, he set fire to his cell in the jail. His memory appeared normal.

Sch. Male, age 42. Constitutional inferiority with episodes of excitement and depression. He attended district school where he was considered far below the average. Previous to admission he had attacks of depression during which he threatened suicide. In one of them he attacked his wife with a knife. He said that something like a voice told him to do it. During his hospital residence he showed childlike docility though periods of depression continued to appear. His memory was good.

Syg. Female, age 16. Constitutional inferiority with episodes of excitement. She attended school eight years but was below normal in her work. Before admission she was frightened by a drunken attack upon her father and for two weeks showed disturbance and fear. Some months later she became disturbed again and expressed fear of personal injury. During her hospital residence she showed periods of indifference alternating with flighty, mischievous periods.

Don. Male, age 22. Dementia praecox, hebephrenic form. About a year before the experiment he began to visit neighbors and interfere with their work. He became at times incoherent and rambling in his speech. He thought a gang was after him. Upon admission to the hospital he showed no insight into his condition. During his residence he was quiet and orderly.

Jack. Male, age 36. Dementia praecox, simple deteriorating type. Attended agricultural college one year. Upon admission to the hospital he was quiet and orderly. He answered questions irrelevantly and was inclined to mutter. His movements were sluggish and he showed no interest in his surroundings. He seemed to be perfectly contented. His memory was good.

Jaco. Male, age 24. Dementia praecox. He attended common school but was not considered especially bright.

Before admission he thought that there was something queer about him and read the Bible to find out about it. There he found passages referring to him. Just before admission he attempted suicide. Upon admission he was alert and pleasant. He laughed while explaining that he sometimes thinks that his father is Jesus and that he himself is the devil.

Har. Male, age 38. Paresis, cerebral form, mildly expansive type. About two years before the experiment he became very forgetful. He would leave his grip or an auto which he had hired and forget all about them. He thought that he was about to receive enormous profits from some investments. Upon admission his pupils were unequal; they showed sluggish reaction to light and prompt reaction to accommodation. He showed a slight Rhomberg, very active deep reflexes, and tremor of extended fingers, of facial muscles, and of tongue. There was also marked speech defect. Both blood and spinal fluid gave positive Wassermann reactions. He showed mild euphoria and a somewhat faulty memory for recent events.

Sau. Male, age 40. Paresis, cerebral form, demented type. About four years previously he began to drink excessively and to keep loose company shamelessly. Upon admission to the hospital his pupils were unequal and they did not react to light or accommodation. He showed very active deep reflexes, marked tremor of tongue and fingers, slight speech defect, and irregular writing. He gave a positive Wassermann reaction to both blood and spinal fluid. He showed a lack of interest in the examination. Later he had the idea that he was to marry W. J. Bryan's daughter. His memory for recent events seemed to be good.

Ev. Male, age 47. Paresis, tabetic form, expansive type. About two years before the experiment he began to talk about schemes to make large amounts of money. Upon admission his pupils were equal but irregular and of the Argyll-Robertson type. His knee jerks were absent and he showed slight speech defect and tremulous writing. Both blood and spinal fluid gave a positive Wassermann reaction. He showed pronounced euphoria and delusions of grandeur. His memory for recent events was poor.

The patients had become accustomed to examinations through those ordinarily given by the hospital physicians and through presentations at staff meetings. They had all been put through the Binet tests which also gave them some familiarity with the experimenter. He frequently met them in the wards and in general cultivated friendly relations with them.

All appeared to have a friendly attitude toward him; and, with a single exception to be noted later, all co-operated very willingly throughout the experiment and showed a distinct interest in the progress of the learning.

The normal subjects consisted of two men and eight women. They were volunteers from a class in introductory psychology.

III. Procedure

The experiments with the insane were conducted in an ordinary patient's room on one of the quietest wards of the hospital. It was believed that such familiar surroundings would be less disturbing to the patients than a special experimental room.

The experiments with the normals were performed in a small room adjoining the main psychological laboratory, at the University of Wisconsin, the size, lighting, etc., being very similar to those at the hospital. The material to be associated, the apparatus and the general technique were identical for both groups of subjects except in a few cases where rest periods had to be permitted to the insane.

Briefly, the plan of the experiment was to have the various subjects form associations to a certain known degree of perfection. The time consumed served as a measure of the rate at which associations were formed. After a week had elapsed they were tested on these associations. The number of associations correctly recalled, together with the number of promptings required for re-learning served as independent measures of the retention. The material consisted of twelve Chinese characters with twelve nonsense syllables (spoken) one of which was associated with each character. The characters were drawn in ink on cards three-quarters of an inch wide which were placed in holders on a drum. This drum revolved step-wise before a small window.[3] The revolutions were controlled by clockwork within the apparatus which was so adjusted that the exposures were of exactly five seconds duration. A character was thus stationary during practically its entire exposure, being suddenly replaced by the next character in the series at the end of the five-second period. The ticking of the clockwork was slow and very quiet, about like an ordinary clock. The subject sat in a comfortable chair before the apparatus, his eyes about on a level with the exposure window, and his back to the window of the room. The

[3] An imperfect model of this apparatus was demonstrated at the Chicago meeting of the Psychological Association in 1915.

experimenter sat on a high stool at the side of the apparatus, facing the subject.

The names of the characters were taught to the subjects by a prompting method. The experimenter called out their names distinctly at the middle of each exposure as indicated by the ticking of the clockwork. The subject repeated the name each time he was prompted. An exact record of the subject's performance was made upon a special blank as the experiment proceeded, a minus sign being recorded each time the subject required prompting, a plus sign where he was able to respond correctly without being prompted, and in cases where an incorrect response was given, he was corrected and the nature of the error recorded. Thus was secured a very complete record of the process of forming the associations. A typical record by a normal subject (No. 7) is shown in table No. III. The incorrect responses are replaced by minus signs.

The learning was continued without interruption until the subject was able to reproduce consecutively the names of the entire list of twelve characters once without prompting. Then the characters were arranged on the drum in a different order and the learning was continued as before until the subject was able to reproduce the series twice in succession without error. At the beginning, subjects were cautioned not to learn the names of the characters in series and the learning of the second order of characters was introduced to secure this more effectually.

Exactly a week after the completion of the learning, each subject was confronted by the characters again in the second (final) order learned, and he was given two opportunities to name each character, i. e., through two revolutions of the drum. At the third revolution the prompting was resumed as before and continued without interruption until the learning had reached exactly the perfection attained the week before, i. e. two successive perfect reproductions.

During the forgetting period of one week, the insane subjects had no reason to expect that they would be called on to reproduce the material learned. It was necessary, however, to tell the normals to return the following week "for another short experiment" which caused a number of them to wonder if they would be asked to recall what had been learned. But all denied consciously rehearsing any of it to themselves.

IV. Results and Conclusions Respecting Memory

The results of the experiments concerning the formation and retention of associations are shown in Tables I and II, for the insane and the normals respectively. A comparison of these two tables reveals two rather striking features which furnish at least tentative answers to the questions with which we set out.

1. It took, on the average, 102 minutes for the insane to form associations which required on the average only 26 minutes for the normals. Thus the insane as a group show a very great disturbance of the ability to receive impressions, requiring on the average about four times as long as normals. Of the disease groups, the three dementia praecox patients make the three best scores, the three constitutional inferiors average next best, and the three paretics average the worst of all.

TABLE I

Showing for the insane subjects the number of Binet tests passed, the number of minutes required to form the associations, and the amount forgotten after one week.

		Intelligence	Minutes required to form associations			Amount forgotten after one week	
Disease	Subject	No. of Binet tests passed	First order	Second order	Total	No. of characters not recalled	No. of promptings to relearn
Constitutional Inferiors	*Bur.*	37	75	14	89	2.0	0
	Sch.	38	124	46	170	4.5	27
	Syg.	35	51	21	72	5.5	26
Dementia Precox	*Don.*	37.5	36	9	45	7.0	21
	Jack.	40	46	6	52	5.0	19
	Vaco.	47.5	29	10	39	1.0	0
Paretics	*Har.*	48	170	30	200	4.0	18
	Sau.	50	45	105	150	2.0	10
	*Ev.**	44	95*	45*	140*	11.3*	258*
Average			72	30	102	3.9	15
M. V.			38	22.6	53	1.6	8.9

* The results of subject *Ev.* are not included in the final averages for reasons given on page 426.

2. In marked contrast to the great disturbance in the formation of associations among the insane, we find that once the associations get formed, they are retained practically as well as by normals. In fact the insane patients are able actually

to recall on the average more of the names of the characters at the end of a week than were the normals, averaging 3.9 failures against 4.7 failures by the normals. But when it comes to the labor of relearning, as might be expected from their more rapid learning in the first place, the normals recover their really greater loss in a shorter time, requiring only an average of nine promptings against fifteen for the insane.

The results of the present study accordingly seem to indicate that at least with samples of the three types of mental disease here investigated, the memory disturbance is primarily one of registration rather than of retention. Their memory for recent events is probably normal if the experience succeeds in making a normal impression.

TABLE II

Showing for the normal subjects the number of minutes required to form the associations, and the amount forgotten after one week.

	Minutes required to form association			Amount forgotten after one week	
Subject	First order	Second order	Total	Number of characters not recalled	Total number of promptings to relearn
1. *Bic*.........	17	6	23	4.0	6
2. *Mc*.........	14	8	22	6.5	24
3. *Bir*.........	16	9	25	6.0	12
4. *Fi*..........	12	8	20	4.5	1.5
5. *La*..........	39	7	46	4.5	12
6. *Ha*.........	19	8	27	2.0	3
7. *Ho*.........	19	4	23	3.5	4
8. *Os*..........	17	11	28	4.5	7
9. *Fr*..........	16	9	25	6.5	12
10. *Pi*..........	18	5	23	5.0	14
Average......	18.6	7.6	26.2	4.7	9.4
M. V.........	4.1	1.6	3.5	1.0	5.25

Two or three details and possible qualifications need to be considered at this point. It will be recalled that in Table I, the results of one subject, (*Ev.*) a paretic, while tabulated with the rest, are not included in the averages. The learning series of this subject was completed successfully, but he strenuously objected to returning a week later to finish the experiment and only went at the emphatic command of the orderly. During the experiment he was sullen, complained of being seriously ill, and obviously made slight effort. He recognized only one of the twelve characters on the test and

consumed 84 minutes in the relearning, requiring over 250 separate promptings in all. His case furnishes a striking contrast to the other abnormals who average only fifteen promptings.

The interpretation of the results in his case are consequently exceedingly doubtful. It is well to remember that this patient had degenerated in intelligence until he was able to pass only 44 of the tests, against a score of 48 and 50 respectively for the other two paretics who showed normal retention. It may also be recalled that a paretic of only a little lower mentality could not learn the material at all. Possibly the retentiveness of paretics becomes disturbed some time after the impressibility has been shattered. It is interesting to note also that this patient had charge of the laundry of his ward and was fairly efficient. On the other hand the paretic *Har.* would very commonly return from an errand to some other part of the building to ask what he had been sent after, having forgotten the errand while on his way to do it. Yet *Har.* gives a smaller record of forgetting than the average normal. The writer inclines to attribute the large score of *Ev.* to possible illness and certain opposition.

The question also arises as to why the insane (with the exception noted above) showed an actually better average score for retention than the normals. In the first place the difference is not great and may have no significance in view of the considerable variation among the insane. The relatively quiet life of the insane might have favored it though all but one of them (*Sau*) performed regular work at the hospital. The long learning periods as such, could scarcely have caused it as it has been repeatedly shown that subjects who take a long time to learn on the whole tend to retain somewhat less than those who learn rapidly.[4]

The most probable explanation seems to lie in the fact that the learning activity produces fatigue which inhibits the power of recall to a certain extent,[5] especially at the end of a protracted learning period. This necessitates a somewhat better learning to produce the required score at the end of a long learning series than at the end of a short one. This may very probably account for the slightly better retention of the insane than of the normals. The experimenter was careful, however, to allow occasional rest periods in the more protracted cases of learning—in two cases rather long one.

[4] D. O. Lyon, "The Relation of Quickness of Learning to Retentiveness", *Archives of Psychology,* No. 34.

[5] E. L. Thorndike, *Educational Psychology,* II., pp. 300ff.

For example, the paretic *Sau,* had a rest period at one time of twenty hours and it is interesting to note that he was able to name correctly more characters after this wait than when he terminated the learning process. During the four minutes preceding the rest he made the following successes: 7, 6, 5, 5; and the first four minutes after resumption: 8, 7, 8, 6. Likewise the other case (*Har.* also a paretic) made an average score of five successes before the rest and of six and one-half successes immediately after resumption.

It is also well known that relearned material is more durable than that only once learned. But this factor must have been slight as the amount forgotten was in each case small, often imperceptible.

V. Fluctuations at the Threshold of Recall

Before the experiment had progressed very far, another marked difference was noticed between the performance of the insane and the normals. This was the greater prevalence among the insane of what may be called "fluctuations at the threshold." What is meant by this may be most easily understood by exhibiting a concrete case (Table III) where the phenomenon appears in a typical form. This is the record of the original learning (first order) of normal subject number seven.

TABLE III

Showing a typical learning record by a normal subject (No. 7).

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
nā 1	−	+	−	−	−	−	−	+	−	−	+	−	+	+	−	−	+	+	+
oo 2	−	−	−	−	−	−	−	−	−	−	+	−	−	−	+	−	+	+	+
vō 3	−	−	−	−	−	−	−	−	−	+	+	+	+	+	+	+	+	+	+
kui 4	−	−	−	−	−	−	−	−	−	−	−	+	−	−	+	−	+	−	+
ling 5	−	−	−	−	−	+	+	+	+	+	+	+	+	+	+	+	+	+	+
fĭd 6	−	−	−	−	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+
tä 7	−	−	−	−	+	−	+	+	+	+	+	+	+	+	+	+	+	+	+
le 8	−	−	−	−	−	−	−	−	−	−	−	−	−	+	+	+	+	+	+
deg 9	−	−	−	−	−	−	−	+	+	+	+	+	+	−	+	+	+	+	+
yer 10	−	+	−	−	−	−	−	−	−	+	+	−	+	+	+	+	−	+	+
nĕz 11	−	−	−	−	−	+	+	+	−	+	+	+	+	+	+	+	+	+	+
chun 12	−	−	−	+	−	+	+	+	−	+	+	+	+	+	+	+	+	+	+

It will be seen at once from this record that frequently a subject will be prompted several times on an association after which he succeeds on it. But after one or two more trials, success will be followed by failure and this in turn

by success and this again perhaps by failure and so on. By the term fluctuation is understood one or more consecutive successes (plus signs) followed by one or more consecutive failures (minus signs). Thus the record of the formation of the first association shown in Table III (*Na*) has four fluctuations—an unusually large number. The first fluctuation begins with trial 2 and ends with trial 7, the second with trial 8 and ends with trial 10. The third fluctuation begins with trial 11 and ends with trial 12, while the last one begins with trial 13 and ends with trial 16.

Now it will be shown later that in order to make a valid comparison of the records of the normals and the insane in this respect, it will be necessary to know more exactly the nature of the phenomenon under consideration. Superficially, at least, the fluctuation appears to be the result of some sort of wave activity (perhaps related to the attention) which is crossing the threshold. Assuming temporarily that this is the case, we shall proceed to examine in some detail the consequences which might be expected to result. Then we shall examine our experimental results to see how far these *a priori* expectations are justified. And lastly if the correspondence is sufficiently exact to warrant, we shall proceed to the comparison of the records of the abnormals with those of the normals upon this basis.

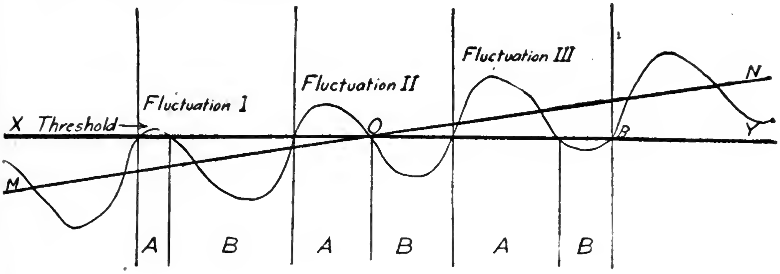

FIG. I

Accordingly in Figure I let the straight line XY represent the threshold of recall; and let the undulating line MN represent any fluctuating process crossing it at a moderate angle. Then obviously, certain things will be likely to follow:

1. The process would be found to alternate above and below the threshold one or more times.

2. In the first fluctuation of a series of two or more fluctuations, the time above the threshold (A) will be shorter than the time below it (B).

3. In the last fluctuation of a series of two or more fluctuations, the time below the threshold (B) will be shorter than the time above it (A).

4. In the middle fluctuation of a series of three or more fluctuations, the time above the threshold (A) will be approximately equal to the time below it (B).

5. If the waves are regular and uniform throughout, the average duration of all complete fluctuations will be approximately equal in whatever position they occur.

6. If, on the contrary, the size of the waves vary considerably from moment to moment, the shorter waves (with presumably shorter period) will tend to intersect the threshold only in the intermediate fluctuation rather than the first or last, thus making the average total duration of the middle fluctuation of a series of three or more, less than that of either extreme.

7. If the direction of the propagation of the wave process should itself curve towards the horizontal so as to make a more acute angle with the threshold as it rises above the latter, (as is usually assumed to be the case in learning) then the last fluctuation would tend to be of slightly longer duration than the first.

8. The more acute the angle at which the wave process crosses the threshold, the height and period of the wave remaining constant,

A. The greater will be the total number of fluctuations at each crossing.

B. The greater will be the length of time between the first appearance above the threshold to the last disappearance below it, i. e., the greater will be the temporal fluctuation span.

C. The longer will be the time required to mount permanently above the threshold.

9. The taller the waves, their period and the angle at which their direction of propagation crosses the threshold remaining constant,

A. The greater will be the total number of fluctuations at each crossing.

B. The greater will be the temporal fluctuation span.

C. The longer will be the time required to mount permanently above the threshold.[6]

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[6] The exact similarity of the results which follow from conditions (8) and (9) will doubtless be noted. This does not mean, however, that the influences of the two factors are indistinguishable from one another. Theoretically at least it is a relatively simple matter to determine for example, how much of the total time consumed in rising per-

We proceed next to see how far our principles developed by *a priori* reasoning are justified by the experimental data. Only two possible cases are open to us. One is to secure the central tendency of the duration of the respective phases of all cases of individual associating processes of two complete fluctuations. The second case is to do the same for all cases of three complete fluctuations. The number of cases of four fluctuations among the normals is too few to possess any reliability, and the extreme variability of the abnormals makes results obtained from them of slight value.

Forty-three cases of double fluctuation were discovered in the records of the ten normal subjects of this experiment, together with the records of twenty other normals who had learned the same material and in the same way but for a different purpose. When added together the total number of successes and failures of the respective phases of each of the two fluctuations are as follows:

Fluctuation I		Fluctuation II	
+	—	+	—
71	88	76	52
$\underbrace{\quad\quad}_{159}$		$\underbrace{\quad\quad}_{128}$	

The totals of fluctuation I clearly accord with principle two, and those of fluctuation II accord completely with principle three. The total time of fluctuation I is distinctly larger than that of fluctuation II, which contradicts principle seven.

We now proceed to the cases of triple fluctuation. In all the thirty odd normal records available for this purpose, only eleven cases of triple fluctuation were found. They are assembled in Table IV and here again the totals will be considered instead of averages.

As before, the relative sizes of the two totals of fluctuation I confirms principle two. Likewise those of the last fluctuation (III) reconfirm principle three. The two totals of fluctuation II while not the same are nearer alike than those of

manently above the threshold was consumed by natural slowness of learning and how much by the fluctuation. In Figure I the entire time consumed in rising permanently above the threshold is XB. If there had been no fluctuations, the threshold would have been crossed for the first and only time at O. Therefore the distance OB is due to the fluctuation and the remainder, XO is due to the natural slowness of learning.

Accordingly, in a given fluctuation record, the part of the apparent learning time consumed by the fluctuation activity ought to be one half of the fluctuation span. If this hypothesis be essentially correct, then this time is really spent in over learning.

either extreme and suggest a confirmation of principle four. The total duration of fluctuation II is less than that of either the first or last fluctuation confirming principle six and tending to reject alternative principle five. Lastly the total duration of fluctuation III is greater than that of fluctuation I thus suggesting confirmation of principle seven.

The coincidences of the experimental results with those of the theoretical analysis while not exact, are nevertheless, very striking. Principles 1, 2, and 3, seem to be amply substantiated; and number 4 also, but with less certainty. Number 6 is confirmed rather than its alternative, number 5, which is what one would expect in a field where practically everything else is subject to variation. The evidence regarding number 7 is conflicting.

Much of the confirmation has necessarily come from the triple fluctuation series which, unfortunately, contains only eleven cases, and naturally its reliability is small. The apparent confirmation may be due to chance, but it is at least interesting to note that if we take separately the totals of

TABLE IV

	Fluctuation I		Fluctuation II		Fluctuation III	
	+	—	+	—	+	—
	1	2	1	1	1	1
	1	2	1	4	1	1
	1	1	2	1	1	2
	1	1	1	1	1	3
	1	2	1	1	1	1
	2	3	1	1	5	1
	1	2	1	1	2	2
	1	2	2	1	2	1
	3	2	1	1	1	1
	1	2	1	2	4	2
	1	1	1	2	3	1
Total	14	20	13	16	22	16
	34		29		38	

either of the two groups of subjects from which they come, all of the significant tendencies appear in each, exactly as when combined. Thus the normals properly of this experiment are:

Fluctuation I		Fluctuation II		Fluctuation III	
+	—	+	—	+	—
7	11	7	9	10	9
18		16		19	

and the subjects from the other experiment:

$$\overset{\textstyle \text{I}}{\underbrace{\begin{matrix} + & - \\ 7 & 9 \end{matrix}}_{16}} \qquad \overset{\textstyle \text{II}}{\underbrace{\begin{matrix} + & - \\ 6 & 7 \end{matrix}}_{13}} \qquad \overset{\textstyle \text{III}}{\underbrace{\begin{matrix} + & - \\ 12 & 7 \end{matrix}}_{19}}$$

Having disposed of the preliminary theoretical considerations, we may now proceed to a comparison of the relative amounts of fluctuation at the threshold found in the normals and abnormals respectively, according to the principles thus rendered probable. As previously indicated, the insane showed vastly more fluctuations than the normals. But it is obvious from the principles worked out above, notably number 8, that we cannot safely take the gross amounts of fluctuation at face value. On the contrary, if it takes one group of individuals four times as long to cross the threshold as another group, then we must expect that the former group would normally show four times as wide a span of fluctuation, assuming the wave activity to be otherwise identical in the two groups. Clearly then in order to compare the normals with the abnormals in this respect, we must divide the fluctuation span of the latter by 4. If after this the fluctuation span of the insane is still twice as great as that of the normals (as it turns out in fact to be) then we may with considerable certainty conclude (according to principle 9B) that the height (or depth) of the wave among the insane, is approximately twice that of the normals.

TABLE V

Showing the index of the fluctuation span and the index of the number of fluctuations for the insane subjects.

Disease	Subject	Index of fluctuation span	Index of number of fluctuations
Inferiors	*Bur*	4.0	.9
	Sch	2.0	.11
	Syg	5.0	.84
Precox	*Don*	2.plus	.55
	Jack	2.4	.35
	Jaco	3.0	.72
Paretics	*Har*	5.7	.63
	Sau	3.2	.6
	Ev	6.2	.89
Average		3.7	.7
M. V.		1.1	.2

As a matter of fact the technique actually utilized was somewhat more refined than that described above, though based upon the same principle. An index of the fluctuation span was computed for each subject by adding all the fluctuation spans found in each subject's record and dividing the total by the number of minutes required by that subject to complete his first learning. A corresponding index of the *number* of fluctuations was also calculated for each subject per unit time. These are shown for the abnormals and the normals in Tables V and VI respectively.

TABLE VI

Showing the index of the fluctuation span and the index of the number of fluctuations for the normal subjects.

Subject	Index of fluctuation span	Index of number of fluctuations
1. *Bic*	2.6	1.0
2. *Mc*	1.0	.5
3. *Bir*	2.25	.8
4. *Fi*	2.2	.6
5. *La*	1.8	.5
6. *Ha*	.8	.3
7. *Ho*	2.3	.8
8. *Os*	1.9	.5
9. *Fr*	2.25	.6
10. *Pi*	1.6	.4
Average	1.87	.6
M. V	.46	.16

A comparison of these tables shows that after the fluctuations of the two types of subjects have been reduced to a common basis:

1. The number of fluctuations at each crossing is about the same with the insane as with the normals, the average being respectively .7 and .6. The larger figure with the insane has slight significance in view of the large M. V.

2. The average span of fluctuation (from the first success to the last failure) is almost exactly twice as much for the insane as for the normals being 3.7 for the former and only 1.87 for the latter. This evidently means (principle 9) that the " height " of the individual fluctuations (i. e. the average differences between the crest and the trough of the fluctuation) is approximately twice as great among the insane as among the normals.

VI. Summary

1. The power of forming associations is greatly impaired in constitutional inferiors, dementia praecox and paretics.

2. This power is far more seriously impaired in paretics of a given degree of dementia than in the other two types of corresponding degree of dementia.

3. The retentiveness of the three types of insane subjects seems to be entirely undisturbed.

4. The fluctuations which occur at the threshold of recall during the process of forming associations appear on the average to follow very closely the law of an irregularly undulating wave crossing a line, the direction of the propagation of the wave perhaps being that of the conventional curve of learning.

5. Having reduced the respective fluctuation records according to this principle, it is found that the number of fluctuation at each crossing appears to be about the same for the insane as for the normals.

6. It is further found that the fluctuation span of the insane is about twice as great as that of the normals. This indicates that the fluctuation waves of the insane are on the average about twice as profound as those of the normals.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

Communicated by E. B. Titchener and H. P. Weld

XXXV. On The Psychological Response to Unknown Proper Names

By E. M. Alspach

This study deals with the first problem proposed by G. English[1] as worthy of additional experimentation; it is a detailed investigation of the reaction of a highly responsive observer to unknown proper names. English had substantiated the results of Kollarits[2] as regards the nature and conditions of the imagery aroused by proper names, but had failed to verify the suggestion of Claparède[3] that there is any community of response to the physiognomy of proper names. Only one of her 8 observers showed any general tendency to be guided in his imagination of persons by the mere sound of the proper name. Further experimentation was necessary, however, before a detailed account of the nature of his response could be made, and this was left for the future.

Fortunately for our study the same observer, the same stimuli, and the unpublished reports of his responses to these stimuli were available. We undertook, therefore, to repeat that part of English's experiment which had to do with the response to nonsense names. We hoped thus to test the stability of the observer's reaction after a fairly long interval of time, to compare the associative factors involved in each group of data, and to obtain fuller characterizations of his meanings and descriptions of the correlated processes.

Method. The stimuli consisted of 50 meaningless words, as follows: Chḗrin, Póisher, Kī́lom, Koikert, Yā́zal, Dáwfisp, Zṓquē, Spren, Dáwthō, Rupzóiyat, Blag, Lísrix, Thaspkū́whin, Kĭ́rdfaumish, Génras, Thā́chō, Brob, Zóitū, Kóldak, Múrbix, Chermtgáwkonv, Bóppum, Vū́shap, Grib, Watshóiquol, Móikī, Hoxzáuwhuk, Gáwthû, Zḗthē, Gówsū, Déznep, Wī́taw, Thṓbonf, Mávquawpŭnt, Stisk, Tówbant, Tā́quû, Skamth, Quajnū́meth, Bū́noy, Drup, Gúklal, Pófmoj, Spux, Jíkzel, Snemth, Thúbtawkarnth, Línrēwex, Gronch, Túpjoz.[4]

The observer sat with eyes closed, and was given a warning signal before the stimulus. The stimuli were presented auditorily; every name was pronounced three times; the experimenter was careful to pronounce it slowly, distinctly, and (as nearly as possible) always in the

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[1] G. English, *Amer. J. Psych.*, 17, 1916, 417f.

[2] J. Kollarits, *Arch. de Psychol.*, 14, 1914, 225f.

[3] E. Claparède, *Arch. de Psychol.*, 14, 1914, 310f.

[4] For the method of obtaining these words, see English, *op. cit.*, 432. Rules of pronunciation are as follows: ă as in at, ā as in fate, aw in awning, au as ow in how, ĕ in met, ē in mete, ĭ in in, ī in fine, ŏ in hot, ō in note, oi in oil, ŭ in tub, ū in tube, û as oo in spoon, ch in church, th in thistle, g in get, j in judge, other consonants as usually in English. Unmarked vowels are short.

same manner. The instructions were: "I am going to speak to you the name of a person; the name will be repeated three times; you are to describe that person to me. After the report is completed, I shall call for introspections." Greater emphasis than before was placed upon completeness of the introspective report. In our experiment, *O* was not asked to repeat the word unless he thought there had been incorrect auditory perception of it.[5]

The observer was Professor W. S. Foster, whose long experience in experiments of similar nature contributed not a little to the success of our investigation.

Results. It may be well to state at the outset that we have here a problem in meaning. *O* is given an intrinsically meaningless word and is instructed to put the meaning-of-a-fitting-person upon it. The attitude, therefore, which *O* takes toward the experiment is a meaning and not a process attitude, and the results are, in the first instance, statements of meaning and not descriptions of psychological processes. Since, however, *O* was also asked to report as best he could the nature of his meanings and the correlated processes, we have a second set of results which are, in part at least, psychological. In our presentation of the results we shall, consequently, attempt to treat separately the two sets of data: first, we shall give a brief account of the general attitude of *O* upon his acceptance of the instruction; secondly, we shall discuss the characterizations of the 'persons meant' in English's experiment and in our own; and finally we shall state the nature of *O*'s meanings and their correlated processes.

(1) *O* characterized his attitude as a literary, imaginative mood in which he was set to construct *the* most fitting person. He understood 'person' to signify 'character,' 'mode of action', as much as (or more than) 'physical appearance'. Furthermore, he was disposed to apperceive correctly the sound of the word, and in case of doubt he pronounced the word before beginning the construction.

(2) *O* was able to construct a 'most fitting person' for every one of the 50 stimuli in two different experiments, *i.e.*, in that of English as well as in our own. Fifteen months had elapsed between the completion of English's experiment and the beginning of ours. *O* recalled the general nature of the previous response in only four cases, and reports indefinite recognition of the stimulus-word in six other cases. Since we desire to institute a quantitative as well as a qualitative comparison of the responses in the two groups, we shall, in what follows, disregard these ten cases.

The Associative Factors which Conditioned the 'Fitting Person'. There seem to be five groups of factors which conditioned or suggested the person that best fitted the name. (1) *Nationality.* In about 30% of cases in A and 15% in B,[6] the word was apperceived as sounding like Russian, Chinese, *etc.*, and the person is described (on the basis of suggestion by nationality) as having characteristics typical of the race. Examples are: *Witaw* (A) "Snap of my eyes toward Siberia and immediately a visual image"; *Hoxzauwhuk* (B) "Said the word over and the word 'Eskimo' came with a visual image of an Eskimo dressed in furs"; *Quajnumeth* (A) "As soon as I heard the word the first time it sounded rather familiar to me, then a snap of my eyes towards

[5] In several cases, even the third repetition did not enable *O* later to repeat the word exactly as *E* had said it. Thus: *Vazal* was repeated as *Bazal*, *Thaspkuwhin* as *Tasquuwhin*, *Chermtgawkonv* as *Tchirkmgonv*.

[6] We shall, hereafter, designate English's experiment as A, and our own as B.

Mexico, a visual image of an old Indian and the attitude which meant that I knew that he was mild, intelligent, and a solid and substantial person". (2) *Similar name.* In approximately 7.5% of the cases in A and 12.5% in B, *O* apperceived the nonsense-name as related to a similar name, and the person seems to have been described, to a considerable extent, from this similar name. Examples: *Boppum* (A) "Said Bottom at once; with a visual image of Bottom as I've seen him in the play with a feeling for all that goes with him. *Boppum* seems even better than Bottom for the character because the p's are fatter and less educated sounds than the t's. Tried to get a different man, but simply got a less definite, fat and brainless one"; *Deznep* (A) "As soon as I heard the word, Desmond, the 'Desperate Desmond' of the funny sheets came up verbally and visually and seemed to fit"; *Zoque* (A) "A visual image of X (a real person of the character and appearance described) whose name is Sophie; and I took up the attitude I feel towards her. I did not say or hear the word Sophie, but *Zoque* at once took on the Sophie meaning". (3) *Auditory-verbal associations.* In about 22.5% of A and 17.5% of B *O* seems to have described the person from his auditory-verbal associations. *Skamth* (B) "Repeated the word, which had the meaning of scamp. At once had the visual image of the person, with the attitude I have for a clever rogue, a sort of admiration-disgust"; *Gronch* (A) "A kind of surly, grouchy, piggish, selfish, self-satisfied beggar; has a thick, heavy mustache; grumbles. Thought of *grouch* and *grunts"; Murbix* (A) "A shady character who is afraid of the police; a sneaky fellow; some rather shallow cunning; does not do things on his own initiative, but would make a good tool for a clever criminal. Repeated the word two or three times and word 'murder' and German 'Mörder' came into my head." (4) *Sound of word.* In 25% of A and 35% of B *O* was not conscious, upon hearing the word, of any such sensible associations, but indicates that it was the mere sound of the word from which he described. *Snemth* (B) "Feeling came at once for a character in Dickens; with a visual image; *Sn* is a mean sound; the shortness of the word seems to signify that you have not much respect for him"; *Mavquawpunt* (A) "Repeated the word a number of times and got the feeling for a jouncing, jumpy movement. The name runs off the tongue like an Indian dance; he was a North American Indian, I guess; at least a barbarian"; *Grib* (A) "Had a feeling for him as quickly as I heard the word; felt *Grib* myself, *i.e.,* obstinate, persistent, muscular, common sense; as if I would fight for anything I thought mine; would be surprised if anyone should rebel against my authority"; *Kilom* (B) "A person who often does ludicrous things; gives me a readiness to laugh but I have no notion why. *Ki* rather than *lom* is important in this desire to laugh; the laugh would be a cackling one since the person is ludicrous; an abbreviation and cut-offness about the syllables that suggests this"; *Poisher* (B) "The striking part of the name is *Poish; P* and *Sh* give smooth and slow and *Poish* all together means big". (5) *Combination of factors.* 10% of the cases of A and 10% of B show responses conditioned by more than one of the above. *Linrewex* (A) "A Welshman; the name is Welsh; but *Linrewex* is light and gives me the feeling of ragtime, so he may be a dancer". (Here there is a definite shift from nationality to mere sound of name.) There are several words in which the observer reports that the different parts of the word are contradictory and seem to lead to contradictory responses. *Thaspkuwhin* (B) "There are contradictions in that word; the nationality part of the word taken alone would give a different person than is described. *Thasp* is a miner; a worker in

metals, pounds with a hammer. *Whin* is a frail man with a high-pitched, nasal voice. *Ku* might be a Chinaman"; *Thubtaukarnth* (A) "*Thub* is clumsiness, thick-fingeredness; might say that a man is *thubby,* meaning that he is all thumbs. *Tau* and *karnth* would signify that the person is accurate and definite"; *Gawthu* (A) "There seems to have been a conflict of determinations this time, for the things that come to me are Dawtho (who was a Greek), a French criminal whose name is like Gawthu; or some Indian; all these things seem to work by selection; might be a policeman;" *Rupzoiyat* (B) "The different parts of the word contradict each other to some extent; the personal appearance is mostly *rup;* his nationality is the whole word."

There remain 5% of A and 10% of B, in which *O* was unable to indicate at once any associative factors; but upon letting his associations run (in a sort of 'psychoanalysis'), he found words which suggest that the disposition for a similar name or an auditory-verbal association may have been effective. *Koikert* (A) "A girl, clerk in a department store. It just struck me now that the sound may have reminded me of 'clerk' though I did not have *kert* meaning 'clerk' in mind before;" *Stisk* (B) "A woman, rather tall, bony; angular, rigid, little bit wooden; a very formal person. It seemed to me later that *Stisk* might be Norwegian for 'wood';" *Tupjoz* (A) "A mighty smart jewel-dealer. This is probably because *Tupjoz* is like 'topaz,' but I did not think of this or know it at the time."

It will be observed that the sound of the name was effective in a much larger percentage of cases than former work would lead us to expect. In the hope of showing the correlation between sounds, on the one hand, and personal characteristics, on the other hand, we have constructed the following table. We have divided the reports into two groups, which we call X and Y. X includes, in general, those letters, diphthongs, syllables or entire words which were designated as broad, big, smooth, slow, *etc.* Y includes the opposite class of thin, definite, snappy, clean-cut, *etc.*

GROUP X

Big	Smooth	Slow	Broad, Generous	Soft
bl	p	p	thŭb	thă
g	sh	sh	p	mŏj
chō	răs	tā́quû	găw	pŏf
	sū	blăg	kŏnv	quăj
			ōw	mĕth
			ō	
			rŭp	

GROUP Y

Snappy	Active	Clean-cut	Definite	Thin	Sharp	Hard
Koikĕrt	zoiyăt	Koikĕrt	gĕn	fĭsp	Kŏldăk	chō
gĕn	kĭrd	Kŏldăk	Sprĕn	Lĭsrĭx	tŏw	Brŏb
Kŏldăk		tŏw	kărnth	ĭn	Brŏb	thăsp
		Kīlom		bĭx	Grĭb	Stĭsk
		Lĭnrēwĕx		rĭn	tŭp	
		kird		whĭn		
				Lĭnrēwĕx		

From the table it is evident that the consonants ch, th, s, sh, p, b, g, j and the vowels u, ow, and ō are characteristic of Group X, while k, d, t, x, n and i, e, a, oi, ŏ are characteristic of Group Y. We refer the reader, on this point, to L. A. Sherman, *Analytics of Literature,* 1893, 21 ff.; L. P. Smith, *The English Language,* 1912, 102 ff.; W. Wundt, *Die Sprache,* I., 1900, 326 ff.

It is possible that this factor may have been effective in other cases, such as nationality, or auditory-verbal association. The observer reported, in fact, that "other things being equal, a long name was a bit important, a short one a little contemptuous". The following examples are typical: *Quajnumeth* (A) "The long name seemed just right for an old man; *meth* is mildness and *nu* is intelligence"; *Dawtho* (A) "It seemed as if the vowel sounds *aw* and *o,* being broad, generous sort of sounds, go with broad shoulders and good nature"; *Koldak* (B) "The man is quick tempered, vicious; jumps when spoken to. The word is like the sharp, clean-cut ticks of a metronone. *Koltik, Galtik, Naltik* could be the name and there would be no difference. *Baltik, Faltik,* and *Maltik* would be different because not so snappy".

Uniformity of Response in the Two Experiments. How did the observer's response to the nonsense-names in the second experiment compare with those given fifteen months before? The answer to this question should give some indication of the stability of the association between name and 'fitting person'. We have, therefore, compared every one of the 40 responses to the same stimulus-word in the two experiments, on the basis of sex, nationality, size, strength, age, characteristics of movements, temperament, intellect, and social status of the individuals described. These categories are necessarily comprehensive. Thus, 'size' includes both height and weight; 'strength' the person's build and muscular strength. 'Age' was divided into 4 groups: (1) up to 17 years; (2) from 18 to 25 years; (3) from 25 to 45 years; and (4) from 45 on. 'Characteristics of movements' embrace quickness or slowness of movement, clumsiness or deftness, general activity, *etc.* 'Temperament' includes good nature or the lack of it, artistic temperament or the opposite, care or carelessness, *etc.* As 'intellect' we considered general efficiency and common sense as well as actual intelligence. 'Social status' was often determined by occupation. In some of these categories reports are often lacking; and in consequence of the variety of descriptive terms used, interpretation and numerical determination of the correspondences and non-correspondences between the first and second descriptions are often impossible. In the following table, Group I comprises the total number of times any single category, such as age, sex, *etc.,* was given in Experiment A; Group II, the total number of times the same category was given in Experiment B; Group III, the total number of times the category was given, for the same name, in both A and B; and Group IV, the number of times that the response to the same name corresponded. By a simple subtraction. then, the reader can estimate the number of non-correspondences between the two experiments.

A consideration of this table shows two things: (1) *O* tends to give certain categories more often than others in both experiments; *e.g.,* sex, age and size are given much more often than strength and intellect. (2) There are more correspondences than non-correspondences in all categories except nationality, 7 *versus* 10; temperament, 6 *versus* 11; and social status, 3 *versus* 8.

The correspondences, worked out in percents, give an average of 61%, with a mean variation of 18%; and a range from 100% correspondence for strength to 27% for social status. Since the chance corre-

spondence could never be more than 50% (sex), and in most of our cases would be very much less (*e.g.*, social status), it appears probable that a fair degree of significance attaches to every one of our correspondences.

Group I (Given in A)	Group II (Given in B)	Group III (Given in A and B)	Group IV (Corresponds in A and B)
	Sex		
39	39	38	24
	Nationality		
29	23	17	7
	Size		
23	32	16	10
	Strength		
9	12	3	3
	Age		
37	38	37	30
	Movements		
15	15	5	3
	Temperament		
24	29	17	6
	Intellect		
14	14	5	4
	Social Status		
21	20	11	3

(3) *Type of Meaning and Correlated Processes.* The meanings of which *O* was aware and from which the most fitting persons were constructed were vague and indefinite rather than concrete, and our account of them will of necessity be general rather than detailed. The course of the meaningful experience was somewhat as follows. The word had the meaning of a person put upon it by the instructions; when heard, a new meaning (that of the 'most fitting person') was immediately given it by way of the determining factors which we have listed above. The new meaning was of the type which Messer has described as a *Sphärenbewusstsein,*[7] and which *O* in other experiments has described as 'the stimulus-object is this kind of feel'.[8] This 'feel' always pointed in a certain direction; if, *e.g.*, the associative factor was 'na-

[7] A. Messer, Experimentell-psychologische Untersuchungen über das Denken, *Arch. f. d. ges. Psychol.*, 8, 1906, 77-80.

[8] H. P. Weld, Meaning and Process as Distinguished by the Reaction Method. *Studies in Psychology: Titchener Commemorative Volume,* 1917, pp. 181-208.

tionality', then the meaning pointed in the direction of 'a person of that nationality'; if the association was by way of a similar name, then the 'feel' was for a person like him who has the similar name. Furthermore, the 'feel' was often empathic, and the empathy was of three forms. In some cases, the 'feel' was *for* the person, *i.e.,* as *O* would feel if he were in the presence of the person; in others, it was *toward* the person, *i.e.,* as *O* would feel when thinking about the person; in still others it was *of* the person, *i. e.,* as *O* would feel if he were the person. There were times, however, when *O* finds it difficult to state the direction in which the 'feel' points: "Part of the time I can't say myself which the attitude is: toward the sound of the word altogether, toward the person, that which the person himself would take, or a combination of all of these. I think that always it had a part of the first in it; it is an attitude that the sound of the word stirs up. Sometimes I think of the sound of the word merely as sound, sometimes of the length or the kinaesthetic facility (or the opposite) of the word. The position of the face, vocal cords and body is expressive of feeling; sometimes there is the attitude which is the meaning of real words of a sound similar to the one in question."

It is characteristic of *O* that this meaning almost invariably precedes the description of the 'fitting person'; only occasionally does the sound of the name at once touch off a verbal description or an object-meaning (visual image of the most fitting person). When the visual image was present without a definite feeling, there was a tendency to let the associations run and to justify the visual image, and the verbal description seemed then to make for definiteness. In the great majority of cases, however, *O* begins his description when he "knows what sort of person fits", when there is formed a "conscious attitude" representative to some degree both of the stimulus-word and of 'that sort of person'. He says: "The feeling seems to touch off scraps of visual imagery and throw me into a slightly more definite kinaesthetic attitude and so touch off the description". Again, "The whole thing of what to say seems complexly determined; but seems to be summed up in a 'feeling.' The description seems to be a sort of explication of the feeling or of the visual image touched off by the feeling."

On the side of correlated processes, *O*'s conscious attitude was partially analyzed by a description of definite kinaesthesis, organic sensations, *etc.* These processes were usually indicated as the basis of expressive movements when *O* put himself in the place of the person described: *Bunoj* (A) "A saucy, impudent person whose nationality I do not know, but I do know that he stirs me up to antagonism and that I do not like him. I puckered up my face and almost got angry and aggressive"; *Skamth* (B) "Scamp. My attitude is that which I have for a clever rogue; a kind of admiration-disgust; the attitude of pushing out of face and lips and an intense stare"; *Witaw* (B) "A visual image of a dog; tended to set teeth or snarl"; *Blag* (A) "Feel *blaggy* myself; as if to say 'Oh, pooh, that isn't right'"; *Dawfisp* (B) "An unemotional person; chilly; either a man or a woman; feeling was to put arms out and drop head on side, expressive of off-hand, not especially interested attitude"; *Chermtgawkonv* (A) "Gives me definite bodily sets". Sometimes the kinaesthesis was definitely verbal: *Lisrix* (A) "Slips easily off your tongue; no effort; do not have to pay much attention to saying it"; *Zau* (*Hoxzauwhuk*) (A) "Is like 'whoosh', the sound that is made by something going through the air; gave me the notion of 'zouee'"; *Mavquawpunt* (A) "Repeated the word a number of times and got the feeling for a jouncing, jumpy

movement". The specific meaning of 'nationality' or of 'that particular person' was usually carried by eye-kinaesthesis; a movement of the eyes in the direction of the country or of the place associated to the particular person. Visual imagery served in making more concrete the nature of the fitting person.

We conclude that in the case of this 'highly responsive observer' the sound of the word is a determinant of the 'most fitting person' in about 30% of the cases, and that on the whole the correspondence of his responses to the same name is considerably more than chance would allow. The characterization of the fitting person is usually preceded by a meaning of an attitudinal sort which points toward or in the direction of the fitting person, and the characterization itself may be regarded as an explication of this meaning. Kinaesthetic and organic sensations are reported as the correlates of the meaning, and verbal and visual processes carry the more concrete meanings which present themselves in the course of the explication.

XXXVI. The Psychological Basis of Appetite

By E. G. Boring and Amy Luce

The psychological establishment of hunger as a kinaesthetic complex of pressure and pain and its physiological ascription to certain 'hunger-contractions' of the stomach[1] led the early investigators to distinguish hunger from appetite, which was used as a term to denote a desire for food, occurring in the absence of sensory, muscularly conditioned, stomachic hunger.[2] Since the taking of food in the early part of a meal inhibits hunger-contractions and the correlated hunger-sensations, the desire for food which persists to the dessert can not be hunger and has been put forward as the type of appetite. There are various ways in which this desire might be psychologically mediated: the appetite-meaning might consist of an attitude toward food, as would be the case if it consisted merely of a sensorimotor disposition to take and eat food; or it might be carried by specific ideas which involve a food-reference; or it might reduce to a particular pattern of sensations or a particular quality of sensation which, as pattern or quality, is itself appetite.[3] We should have, in the first case, an action-consciousness and, in the second case, an ideational consciousness, of which neither would be psychologically peculiar; appetite would then possess individuality of meaning but not of mental process or pattern. In the third case we should have a truly psychological distinction It is some such sensory account of appetite that Carlson appears to imply when he states on the basis of experimental work, that "the gastric mucosa is

[1] A J. Carlson has summarized this experimental work in *The Control of Hunger in Health and Disease,* 1916; see esp. pp. 16-83.

[2] W B. Cannon and A. L. Washburn, An Explanation of Hunger, *Amer. J. Physiol.,* 29, 1912, 441; Carlson, The Relation between the Contractions of the Empty Stomach and the Sensations of Hunger, *ibid.,* 31, 1913, 185ff.

[3] *Locc. citt.;* E. G. Boring, Processes Referred to the Alimentary and Urinary Tracts: A Qualitative Analysis, *Psychol. Rev.,* 22, 1915, 315f.

endowed with a protopathic sensibility" which constitutes a "gastric component of the sensation of appetite."[4]

Carlson puts forward a specific method for obtaining these gastric sensations of appetite and for distinguishing them from hunger. "Moderately cold water, beer, wine, weak acids (0.5 per cent) or weak alcohol" introduced into the stomach through a stomach-tube "give rise to a characteristic sensation which fuses with, or cannot be distinguished from appetite." "By introducing these substances through the stomach tube at the height of a gastric hunger contraction one actually experiences a *successive contrast of the sensations of hunger and appetite,* as these substances temporarily inhibit the hunger contractions in stimulating the gastric mucosa. . . . From the first it was clear that, when beer, cold or hot water, were introduced into the stomach during a vigorous hunger contraction, the sensation resulting was the exact opposite of that caused by the hunger contraction. In place of an unpleasant, tense sensation, associated with restlessness, the sensation caused by these different stimuli is one of relief. A pleasant tingling sensation is felt in the stomach. One feels perfectly at ease, but the thoughts tend to revert to the dinner table. At first he [Braafladt] was not able to say just what this sensation was like, although it was a familiar one. After paying close attention to the sensation experienced at meals just after a few mouthfuls of food or drink have been swallowed, he became convinced that the two sensations are very much alike if not identical."[5]

Carlson writes as a physiologist and goes no farther than to set the psychological problem. He tells us that appetite is, in part, sensory, but he does not tell us whether the appetite sensation is a unique quality, or a unique spatial, temporal, or intensive pattern of familiar qualities, or simply a distinct meaning which occurs without psychological uniqueness.[6] It was our intention in this study to apply Carlson's method and to obtain a psychological description of the experience which he has called appetite.

Initial Experiments with Carlson's Method. Through one of our stomach-tubes (outside diam., 9 mm.; lumen, 6 mm.) we passed a much smaller tube (outside diam., 4 mm.; lumen, 2 mm.), which projected a few mm. beyond the larger tube and bore on its end a rubber balloon. When this rubber balloon was inflated in the stomach and the external end of the small tube connected to a tambour, we could record stomachic contractions upon a kymograph. The large tube at

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[4] Carlson and L. H. Braafladt, On the Sensibility of the Gastric Mucosa, *Amer. J. Physiol.,* 36, 1915, 162ff.; Carlson, *The Control of Hunger, etc.,* 96-118. The use of the term "protopathic" is, in this connection, peculiarly uncritical. Head, who invented the term, had nothing to say of appetite. These authors apparently accept Head's dictum that the internal organs do not mediate "epicritic" sensibility, a position which leads them into incompatibilities; see Boring, *Psychol. Bull.,* 14, 1917, 99. And why, furthermore, are we to bestow these newly discovered internal sensations upon "protopathic" rather than upon "deep sensibility"?

[5] Carlson and Braafladt, 162f. The method was demonstrated before the American Biological Societies: Carlson, *Amer. J. Physiol.* 33, 1914, xv.

[6] By the word "sensation" Carlson does not intend to imply psychological simplicity. For example, he uses "hunger sensation" and "hunger sensations" interchangeably (*Control of Hunger, etc.,* 62-71) and appears at the same time to admit the complexity of hunger (p. 26).

its outer end was attached to a device for introducing liquid stimuli[7] under slight pressure; these stimuli could be passed to the stomach inside the large tube and outside the small tube, while the stomachic contractions were being recorded.

Our observers were Dr. E. G. Boring (Bo) and Mr. F. L. Dimmick (D), who had had previous practice in similar experiments,[8] and Miss J. M. Gleason (G).

We had little difficulty with these observers in getting records of typical hunger-contractions with contemporary introspective accounts of the hunger-complex. We were thus assured of the familiarity of our observers with the introspective nature of hunger.

As stimuli we used 5% alcohol, 0.5% HCl, and beef tea. These solutions we introduced in varying amounts (50 cc. to 150 cc.) into the stomach at times when the kymograph tracing indicated strong hunger-contractions. The observers were instructed to give a description of all processes referred to the stomachic region.

This experiment failed of its original intention, because it brought out no organic experience contrasting with hunger or differing markedly from hunger. We did obtain descriptive norms for hunger. When the stimulus was introduced the hunger and the hunger-contractions were sometimes, although not invariably, inhibited. When the hunger was inhibited, there seemed to follow no specific sensory pattern; there was nothing which could be called appetite. All that was left were weak, vague pressures and pains, which passed at times gradually over into a mild hunger. The following reports are typical:

Bo. "Hunger was inhibited suddenly. Then for a moment it was just pressures. Then cold in stomach and throat. Then hunger back again. Then a vague small hard little ache, which seems static for a moment and then begins to fade and fluctuate and spread, and finally turns into a real hunger ache."

D. "The burny pains of hunger left; other pressures stayed. And the gnawing thing went. Later the bright, burny thing came back, after the tube was out, and was more diffuse."

G. "Had heavy pressure [=hunger]. When alcohol entered, the pressure left. Cool in larynx and at place almost to sternum. Then gradually hunger came back very slowly."

On the nature of hunger as a complex of pressure and pain,[9] we may note these reports:

D. "Characeristic thing about hunger is a little bright burny thing at the base of the stomach. It is a pain, although it would not ordinarily be called so. There are other diffuse pressures mixed with this." "I have a painy thing that comes and goes. It comes slowly, welling up, then dies out quickly. That is the central part of hunger. This pain is a funny sort of burny thing."

G. "Hunger is just a pressure, constant, which varies in intensity and is very profuse. And there is a pain which is the same as the pain in extreme pressure. This dull pain comes sometimes for fifteen or thirty seconds."

Bo was familiar with hunger as a complex of kinaesthetic pressure and pain on the basis of other experiments,[10] and did not attempt to describe it separately at this time.

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[7] *Amer. J. Psychol.*, 26, 1915, 8f. and Fig. 1.
[8] *Ibid*, pp. 5-55, 485-494; *Psychol. Rev.*, 22, 1915, 307-330.
[9] Boring, *Psychol. Rev.*, 22, 314f.
[10] *Loc. cit.*

Experimental Meals with a Group of Observers. Our failure to find appetite with Carlson's method led us to look for it under the more normal conditions of the meal.

We began with group meals; for these we added to the three observers already mentioned four others: Mr. H. G. Bishop (Bi.), Mr. C. N. Clark (C), Mr. G. J. Rich (R),—all graduate students,—and Miss A. Luce (L). Every observer was given a note-book in which to write his reports.

In the first experiment these general instructions were given: "Whenever instructed to do so, you are to make in this note-book a report in writing, which shall involve the following points: (1) Are you hungry now? (2) Do you feel any desire for food other than hunger? (3) If you do not have a desire for food in general, do you feel a desire for any particular kind of food? (4) Give a description of any sensory complexes (organic 'feelings') which appear to come from your alimentary canal (mouth, throat, oesophagus, stomach), including any sensations involved in hunger or 'desire for food.'" By these instructions we hoped to bring out appetite, and to get it described in relation to hunger, without directly suggesting Carlson's conclusion to our observers.

The observers were asked under these instructions to write an initial report. Then steaming soup was brought into the room and the odor allowed to diffuse, so that hunger might be accentuated, and a second report was called for. Then the observers ate a few crackers to inhibit the hunger and reported a third time. Then they were told to eat soup and bread "until you have had enough." When they indicated that they had had sufficient soup, crackers and cheese, olives, Nabiscos, and peppermints were set before them, and they were told: "Pick out some article of food which you would like to eat now. Before you eat it, compare your feelings when contemplating eating it with your feeling when contemplating eating more soup, and report on these feelings according to the general instruction."

The results of this experiment indicated that the observers could be trusted to select their own times for reporting, and that they needed to be encouraged to report meanings as well as to describe sensations. Accordingly, for the second experimental meal we used the following instruction: "You are to write in this book successive reports which shall deal with the 'attitudes' and mental processes immediately involved in eating this meal. You should report all *attitudes* or *meanings* that pertain to food-taking (*e.g.*, hunger, desire for food, revulsion, *etc.*) and all *mental processes* (particularly those sensations which are referred to the mouth, throat, oesophagus, and stomach) which underlie these 'attitudes' or meanings. You are to make a report whenever there is a marked change in the 'attitude' or in the sensory complex referred to the alimentary canal. It is suggested that you will probably wish to make *at least* three reports: one before the meal, one after taking a little food, and one toward the end of the meal."

We may note in the first place that our observers had no difficulty in differentiating between hunger and the 'desire for food' or appetite.

Bi. "Hunger has a keen edge of abdominal pressure. Appetite loses the unpleasant accompaniment of hunger. Instead of the pressure and pain of hunger there is a squirming light pressure in the pit of the stomach and increased salivation."

Bo. "Hunger is a dull, slowly fluctuating ache in the stomach region. Appetite is a tactual sensitiveness of my mouth and to a less extent of my oesophagus. Partly mouth watering." "My alimentary canal is 'alive', especially achy (not hunger) near my stomach, and pressury

in my mouth (saliva)." "The big outstanding thing to my 'appetite' is a stomach-ache. It is more lively, of less body, and more easily localized and attended to than is the hunger-ache."

C, although possessing normally a strong appetite, was rarely hungry.

D. "The desire for food is continuous and is therefore something in addition to the sensory thing I call hunger. Hunger is a vague mixture of pressures and a peculiar sort of 'pain' in the region of the stomach." "After eating a few crackers, there was a lull in the hunger sensations, but the desire for food was still present. This desire is a rich complex, and consists largely of mouth and nose components with ever changing visual imagery."

G. "In my stomach a dull extended muscular pressure means hunger; whereas dryness and warmth in my mouth and throat (except when my mouth waters as it does when I get images of food) mean 'desire for food.'"

L. "Appetite is like hunger, but is more mild. It is a very diffuse ache in the region below the sternum." "When I think of eating the mint my mouth has a peculiar tactual sensation and waters somewhat."

R. "Hunger consists of an achy pressure. . . . After taking a few crackers, this pressure-ache tends to disappear and to become spasmodic. Just below it in the body there appears a light tingling pressure. . . . When contemplating olives, I desire to eat them, and I find a faint tingling in the pit of the stomach. The pressures seem to fade out of consciousness while the tingling sensation comes to the front."

We conclude that appetite is not hunger. It may be differentiated from hunger by the nature of certain stomachic sensory components (Bi, Bo, L, R) or by the presence of oral sensory factors (Bi, Bo, D, G, L) or by imagery (D, G). R makes the distinction in terms of stomachic sensations alone; Bi, Bo, and L refer to both the stomachic and oral factors; D and G assign the difference to the oral sensations *plus* their imaginal supplementation.

Already, then, we are gaining a hint of the real nature of appetite. Appetite may involve (*a*) stomachic sensations, (*b*) oral sensations, (*c*) imagery, and, as we shall see later, (*d*) general bodily attitudes or dispositions. Let us take these components up in order.

(*a*) Bi, Bo, C, L, and R report *stomachic* components in appetite.

Besides Bi's report (quoted above) of "a squirming light pressure in the pit of the stomach", he tells at another time of "a heightening of a sinking pressury pain in the region of the stomach", which means appetite. Again: "I get a sort of squirming pressure in the pit of the stomach; it is a light, bright, half-pleasant experience."

Bo gives several reports consistent with those quoted above, which characterized appetite as a 'lively' 'stomach-ache', "of less body, and more easily localized and attended to than is the hunger-ache." For example: "Hunger is gone. There is still an ache in the stomach, but it is almost constant, not so lively, nor so penetrating." And, moreover, appetite "is pleasant, while hunger tends to be unpleasant."

C speaks of a "stomachic sensation of warmth" involved in the desire for food.

We have quoted L's mention of a diffuse stomachic ache, milder than hunger, in appetite. This ache can not be wholly unlike hunger, for it is "a slight contraction-like feeling in the stomach, which fluctuates and is present when I attend to it."

R is explicit in his reference to appetite as "a light, tingling pressure" (see above). This "tingling pressure is localized under the

point of the sternum and further down; it is rather broad and diffuse. I suppose I should simply call it a tingling."

(*b*) Bi, Bo, C, D, G, and L ascribe *oral* components to appetite.

Bi mentions 'mouth-feels' and the flow of saliva. "The feels in the mouth are heightened and the odor of the soup adds to the flow of saliva." "I have a slight flow of saliva as I anticipate the next spoonful."

We have quoted Bo on the "tactual sensitiveness" of mouth and oesophagus and on the salivary pressures in the mouth. Mentions of "mouth sensation including copious salivation" are repeated.

C reports: "At the presence of food, the secretion of saliva increased noticeably. There are sensations of pressure at the base of the tongue." "The appetite remaining seems to be the saliva flow and pressure sensations in the mouth and throat."

D calls the desire for food a "rich complex" which "consists largely of mouth and nose components;" "sniffing and mouth and throat sensations are most prominent."

G describes "dryness and warmth in my mouth and throat, except when my mouth waters. The warmth and dryness in my throat are urgent desire for food, relieved by the moistness." "Mouth waters. Passage seems clear to oesophagus, *i.e.*, relaxed." "With the thought of bread and butter, my mouth and throat seem relaxed and open, *i.e.*, free from strain and pressure."

L writes: "When I think of eating the mint, my mouth has a peculiar tactual sensation and waters somewhat. There is a tactual ache in the back of my throat. When I have a desire for more mint, my mouth waters."

(*c*) D and G, as we have seen, consider *imagery* relevant to appetite.

D reports both "visual imaginal complexes, accompanying" the oral sensations in appetite, and "kinaesthetic images of the puckery and cool feel" which eating olives would give.

G notes that "my mouth waters as I get images of food," and, more specifically, that "it is an image of coolness and smoothness which I get clearly now as I look at bread and butter."

(*d*) Bo and D point out that the desire for food may sometimes be more adequately described as *attitudinal* than as sensory.

Bo writes: "I do not want food violently, but when I think of my alimentary canal I can recognize the impulse for taking food,—a purely motor, determinative thing, that is like the impulse to drink when one is mildly thirsty."[11] "I have the impulse to take food when I think about it in purely kinaesthetic and 'habit' terms."

D says of appetite: "I discovered the dish of olives near by, wanted one, took it, and ate it. The reaching was almost reflex."

Individual Experimental Meals. The group-experiment does not provide the best introspective conditions. Accordingly, we sought to supplement the collective trials by sessions in which single observers dictated their reports to the experimenter. These experiments were made on Bo, D, and G, both when hungry and when replete. When hungry, the observers ate a few crackers to inhibit the hunger contractions. Then, in the presence of olives, or crackers, they were given the following instruction: "Take up an attitude of desiring something to eat. If you succeed in taking this attitude, give me as complete a psychological description of the attitude as you possibly can. Describe any sensations which may be relevant and also characterize the attitude in general terms."

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[11] *Ibid.*, 310.

In these trials we find again that Bo reports a *gastric sensory component* of appetite and that D and G do not. It is true that G once mentions a stomachic pressure, but its description resembles hunger and its relevance is not explicitly stated. Bo's gastric appetite is not, however, hunger. It is a bright, lively ache, brighter and livelier than hunger, with a different temporal-spatial pattern from hunger (small, fixed, constantly persisting), and pleasant; "this pleasantness seems to hang right into the quality in just the way that the pleasantness in the ache of stretching does."

The *oral complexes* constitute the most generally recognized basis for the appetite-meaning. The three observers all make much of the sensations which result from increased salivation. They all also mention pressures and aches in the mouth and throat, similar to the sensory basis of thirst,[12] which mean desire for food. G also mentions coolness of the mouth. D and G imply kinaesthetic factors when they say that the movements of the tongue and jaws are free and easy; and G insists further that she feels her throat relaxed. Bo and D report sniffing, as if to inhale the odor of food.

Imagery is assigned a subordinate rôle. All three observers report it, but D now fails to find it relevant to appetite. Bo and G usually consider it as a condition for the increased salivation of appetite rather than as an intrinsic part of the appetite experience. Certain oral images may, however, be relevant. G reports "a taste image,—salt and olfactory;" Bo says, "When I desire the olives, I can image the pressure-pucker which I get with bitter olives." In any case, imagery is subordinate to the oral complex; either it conditions a factor in the oral complex or else it adds imaginally another oral component.

The *kinaesthetic* factors already mentioned represent a deglutitory attitude. The sniffing, the freely moving mouth and tongue, the relaxed throat, all reflect a readiness for food. G speaks of "motor tendencies, which seem widespread in mouth and jaws." Bo and D extended this disposition to the whole body. Appetite was for them at times a reaching-for-food. D, for example, remarks: "The wanting of the cracker is an unconscious tendency to reach for it. The motor side is the first thing that comes." "The big part of appetite is that I can't keep my hand away from the cup. The whole thing is to move and get an olive." "Appetite is mostly a bodily attitude which involves getting and eating, *i.e.*, the muscular side. Part of the time I feel myself reaching and then I see my hand going. That is *Kundgabe*, which means kinaesthetic feels in my arm. And I have kinaesthetic feels or imagery in my jaw and face, twitches which mean chewing, together with movement of the tongue."

Such reports bring us to a difficulty inherent in our problem. Appetite is a meaning; and, though meanings may be recognized and reported, they can not be described. Our observers had no difficulty in recognizing appetite; but they sought to describe it. If there had been some sensory quality which invariably carried the appetite-meaning, they would have doubtless hit upon this sensation as the essential feature in their description. But, Carlson to the contrary, there seems to be no sensory *sine qua non* of appetite. Thus our observers were forced for description to give various available processes which they regarded as relevant to the appetite-meaning. The question of relevance was not always easy for them to decide, and the attempt to make the decision led them often to a report of attitude and away from description. The following protocol of Bo illustrates the variety of reported factors,

[12] *Ibid.*, 310f.

the difficulty of determining their relevance to appetite, and the lapse from description to attitudinal report, in order adequately to account for the appetite-meaning:

"The prominent thing in this appetite is stomachic sensation. There is a more or less static fixed pressure-ache complex, in which the ache is not so dull as it is in hunger, which lacks the intermittence of hunger, and which penetrates into me more. This penetration is a sort of thrust-up, and perhaps arises because my attention tends to shift to my throat, just as is the case with the stomachic temperature sensations which I may localize either in stomach or throat. I don't entirely localize these pressure-aches in my throat, because when I attend to my throat the ache drops out; but the throat-pressure and the stomach-ache do seem to be related because attention shifts back and forth from one to the other so readily. I don't think that hunger leads to the throat in the same way. . . . Then there are also the pressure mouth sensations, involved in copious salivation; both the pressure under my tongue and the feel of the top of it are altered. . . . Both mouth and stomach complexes seem, under critical survey, to be nothing but sensations and scarcely to account for the desire for food. What happens is that, as I look at the crackers and work myself into the situation, sometimes the mouth sensations or more often the stomach sensations get clear, and then all at once I find myself incipiently moving to take a cracker. It seems as if either the stomach or the mouth could mean 'Take a cracker!', and that under the psychological attitude they turn into mere sensations, which are very different things from a desire. . . . As I sit here attending to my report, all at once the stomach thing wells up and gets to be nearly the whole of consciousness; it is immediately supplemented by an attitude of activity which means that I just want badly to pick up a cracker and eat it. If I let my mind dwell on picking up the cracker and eating it, my attention goes back to my mouth, and I find my mouth watering vigorously. The whole experience is analogous to being tickled under psychological conditions. The sensory content of tickle is just sensory content, and the squirming to get away seems perfectly unaccountable and mechanical. But at a commonsense level I feel as if the tickle caused the squirming."

Retrial of Carlson's Method. In our initial experiments with the stomach-tube we failed to find an appetite-sensation. In the experimental meals, however, five observers (Bi, Bo, C, L, and R) reported stomachic components of appetite, whereas only two observers (D and G) did not. It seemed reasonable, then, that we might succeed in isolating a stomachic component if we returned, after the introspective practice of the experimental meals, to the method of the stomach-tube. Since the balloon in the initial experiments had been uncomfortable, we abandoned it in these trials and therewith the record of gastric contraction. We worked with the three observers who had learned to swallow the tube, Bo, D, and G. Warm tomato soup had proved adequate to appetite in the experimental meals; we, therefore, employed it here, in doses of 400 and 200 cc., slowly administered through a funnel. The temperature was such that it gave rise to few thermal sensations. Repletion was produced by the larger dose, but not appreciably by the smaller. The sessions were held when the observers were hungry, and accounts of stomachic sensations, consisting principally of descriptions of hunger, were taken before the tube was introduced. The tube was swallowed to a point 30 cm. from the teeth.

The instruction, given after swallowing the tube, was as follows:

"Take note of the sensations or sensory complexes of stomachic origin which you now have. Then, when you are ready, signal to the experimenter for the stimulus. After the stimulus has been introduced, you are to note again the stomachic sensations or sensory complexes. When you are ready, remove the tube and give as completely as possible a description of the sensory material which you noted both before and after the stimulus."

In the first place, we may note that the introduction of the soup was followed for all observers by the cessation of hunger (inhibition of hunger-contractions, presumably), and that the disappearance of the hunger was followed for Bo and D and probably for G by a short period of 'blankness', during which there appeared to be no sensations referred to the stomach. Bo reports once, as the initial phase of the period, "Nothing;" and in another trial, "After the stimulus the stomachic sensations disappeared entirely. I was completely blank except for the throat sensations from the tube. After all the soup was in, I could still feel no properly stomachic sensation. I am very positive about this." D says: "The first thing I noticed was an absence of sensation as compared with the complex I had before;" "the region now is characterized by its blankness." G notes that "when the soup went in the muscular pain moved all around and then disappeared." We do not find, then, the "successive contrast of the sensations of hunger and appetite," which Carlson has led us to expect.

All observers reported an experience of 'fulness' or repletion, which occurred with the larger dose, and which they described in terms of pressure with sufficient accuracy to distinguish it from the complexes to which we must now turn our attention.

We have seen that under the appetite-situation of the experimental meals Bo tended to report stomachic components, and that D and G practically never did. A similar individual difference holds in these trials. Bo, following the period of 'blankness', finds the live, bright stomachic pressure-aches, which he had formerly designated as a component in appetite, and he is able, although with difficulty, still to attach the appetite-meaning to them. D and G, under these conditions, neither report appetite nor find the bright pressure-aches which somewhat resemble hunger; on the other hand, they are not entirely without stomachic sensations. After the lapse of the period of 'blankness', they find pressure-complexes recurring in the stomach, which indicate that the stimulus may be adequate to some sensation, even if not to the meaning of appetite. The pertinent quotations follow:

Bo: "After the blankness, a pressure-pain, like the hunger-complex, only less intense, not so big, more definitely localized, and of approximately constant intensity. I tried to ask myself whether this was appetite; it did not have the meaning of craving for food that hunger had. It was more a liveness of the stomach region, which means, 'I'd be glad to have more food'. . . . Later this pressure-ache again, on which I could put the appetite meaning voluntarily. . . . Altogether, however, these feelings do not take on gastronomic meanings readily. I could call this 'experimental appetite', meaning that it is not the prominent part of the usual appetite-experience."

D: "Now after the period of blankness I notice a lower pressure, a different pressure from those of hunger and of repletion, which is more superficial. . . . Later the pressures of the first part of the experiment have gone."

G: After the dying out of the hunger pains "I got a diffuse muscular pressure, much less intense than muscular pressure usually is. It seemed undulating and more intense at some times than at others. It

narrowed up. It did not change into pain at all. It persisted as diffuse pressure."

PSYCHOLOGICAL CLASSIFICATION OF THE PROCESSES WHICH CARRY THE MEANING OF APPETITE

		1	2	3	4
		Sensory stomachic complex	Sensory oral complex	Imagery	Kinaesthesis of general bodily attitude
Regular	Bo	Yes	Yes	Sometimes	Yes
	D	Probably not	Yes	Rarely	Yes
Observers	G	Probably not	Yes	Sometimes	Yes
	Bi	Yes	Yes	....	....
Occasional	C	Yes	Yes	....	....
Observers	L	Yes	Yes	....	....
	R	Yes	....	....	Possibly

Conclusions. Appetite can be adequately described only as a food-seeking attitude or meaning, a reaching-out-after-food.

A psychological account of appetite attempts to describe the sensory processes correlated with this attitude. In the large they consist in the general kinaesthesis of the orientation of the organism toward food (muscular activity, automatic movement kinaesthetically sensed, *etc.*). The psychological content is as meagre with respect to the total meaning as is the bare content of typically attitudinal experiences like tickle [13] and thirst.[14] (See column 4 of Table.)

The most constant and characteristic feature of appetite is an oral, deglutitory attitude, which involves sniffing, free movement of the mouth and tongue, copious salivation, and relaxation of the throat.[15] The corresponding sensory pattern is a predominantly kinaesthetic complex of pressures and aches, supplemented occasionally by warmth or cold, and reflecting in its specificity the oral attitude. Psychologically it resembles the sensory pattern of thirst,[16] although markedly different in meaning. (See column 2 of Table.)

Imaginal processes are relevant only as they supply oral images or condition the salivary reflex.[17] (See column 3 of Table.)

[13] E. Murray, A Qualitative Analysis of Tickling, *Amer. J. Psychol.*, 19, 1908, 289, esp. 329f.

[14] Boring, *Psychol. Rev.*, 22, 310f.

[15] Possibly also initial movements of deglutition; *cf. ibid.*, 315.

[16] *Ibid.*, 310f.

[17] Cannon and Washburn, therefore, overestimate the importance of imagery in appetite; *loc. cit.;* Cannon, *Bodily Changes in Pain, Hunger, Fear, and Rage,* 1915, 233f.

Food in the stomach may give rise to sensations which do not constitute hunger. These sensations are described as bright, lively, tingling pressures or aches, which are pleasant, and, unlike hunger, static, definitely localized, and easily attended to. Some observers (D, G), however, do not have these sensations; with food in the stomach they report either no sensations at all or vague, indefinite pressures.

The bright, lively, tingling, stomachic complex fuses readily with the oral pattern in carrying the meaning of appetite (Bi, Bo, C, L). It is not, however, essential to the appetite-meaning (Bi, Bo, D, C, G, L), although one observer (R) found that, unsupported, it carried this meaning; and another observer (Bo), that the appetite-meaning could voluntarily, although with difficulty, be added to the isolated stomachic complex. Two observers (D, G) never based appetite upon stomachic sensations. (See column 1 of Table.)

It seems reasonable to suppose that this bright stomachic complex is the same as the "sensibility of the gastric mucosa" described by Carlson. In interpreting this pattern as an essential component of appetite, Carlson and Braafladt presumably put upon it their own individual meanings, which are, apparently, not completely in accord with those of other observers.

Against Carlson we would urge that the stomachic sensations are not always prominent, nor even always present, in appetite; that, when present, they frequently contribute but scantily; and that, when present in isolation, they often cannot mean appetite. It may be that appetite, as meaning, contrasts with hunger; with respect to stomachic sensory pattern, on the contrary, it resembles hunger, though not identical with it. Furthermore, the aspect of appetite does not appear immediately upon the inhibition of hunger by food, but after a short interval.

Finally, we may point out that confusion would often be avoided if the physiologist were to distinguish between the attitude that, as biological meaning, appears unitary, and the psychological sensation which is an observational element. Unitariness of the former may be correlated with complexity in the psychological sphere, and there is no reason to assume that every biologically simple experience has its corresponding single sensation or afferent process.

ON THE COMPUTATION OF THE PROBABLE CORRECTNESS OF DIFFERENCES

By Edwin G. Boring

One of two points of disagreement between Prof. Fernberger[1] and myself [2] is that, while I say that the significance of a difference between two limens or between a test-case and a norm must depend on the probable errors of each of the measures compared, he evidently believes that a significance may be reliably indicated when the probable errors are not available. And of course he is right, for practical instinct is not necessarily based on theoretical conviction. If—to take his instance of Grabfield[3]—we know that a faradic limen above 140 *B*-units has never been found in normal subjects, and that pathological subjects give limens from 120 to over 400 *B*-units (av. = about 200), then the clinician, who finds a threshold of 200, concludes with reasonable certainty that the case is pathological, although he may never know the P.E. of his subject, or of pathological subjects in general, or of the norm. How, then, does the clinician make his diagnosis?

Let us see first what theory requires. A norm is generally based upon many observations upon every one of many individuals. Thus it involves two modes of variability: an inter-individual mode, which is measured by the P.E. of the individual averages, and an intra-individual mode, which is measured by the average of the P.E.'s of the individuals. In theory we must take both into account, as we can by considering a new P.E. of all observations without regard to the individuals, *i.e.*, by taking the P.E. of a sort of 'group-individual.' In the diagnosis of a single subject, we must then consider both these modes of variability: we must find (1) the probability that we have not an instance that is exceptional for the particular individual, and (2) the probability that the individual's average is not merely an unusual case within the normal range of variability of individuals. Actually, we should have to find, by repeating the determinations of the threshold, the individual P.E. of our subject, and then throw that P.E. over against the P.E. which combines both modes of normal variability, before we could determine the probable correctness of the departure of our subject from the norm. If we can not take time to determine our subject's P.E., we may perhaps *assume* that his P.E. is no larger than the average P.E. for normal and pathological subjects, and then use some such measure in determining the P.E. of the difference. If the intra-individual P.E. is known to be fairly constant in all cases, we

[1] S. W. Fernberger, Concerning the Number of Observations Necessary for the Determination of a Limen, *Psychol. Bull.*, 14, 1917, 110.

[2] E. G. Boring, The Number of Observations upon which a Limen May be Based, *Amer. Jour. Psychol.*, 27, 1916, 315.

[3] G. P. Grabfield, Variations in the Sensory Threshold for Faradic Stimulation in Psychopathic Subjects, *Bost. Med. and Surg. Jour.*, 171, 1914, 883; a clinical article, unsatisfactory to the psychophysicist on account of the omission of data.

may perhaps make this assumption with validity; otherwise we must—there is no escape—determine the P.E. of the individual.

The instinctive diagnosis of the clinician means that he knows approximately what degrees of variability he may expect, and that instead of making nice calculations, he simply adopts a rule of refraining from positive diagnosis except when the difference is so great as to be for him unequivocally significant. To state that a faradic threshold greater than 175 *B*-units is "definitely pathological" is to imply a norm, an intra-individual and an inter-individual variability of that norm, and a pathological intra-individual variability. The clinician works implicitly where the psychophysicist works explicitly. Both may be right, but in the doubtful case the odds are with the psychophysicist.

On the other hand, unnecessary labor is always futile, and there are times when the "compromise between time and accuracy," which Fernberger urges, may gain for us more than it loses. If the psychophysicist can determine for the clinician a certain value as a *differentia* (like 175 *B*-units), which has, for known variabilities of the measures involved, a given probable correctness, then he saves the clinician's time. Moreover, there are ways in which he can save his own time.

In the first place, he may utilize that respectable mathematical notion of the negligible quantity. Fernberger laments that "the work of obtaining the probable error of this average threshold would be a very laborious affair." But perhaps it need not be determined; perhaps we may neglect it, that is to say, call it zero. The P.E. of a norm depends on the number of cases involved. If, for example, a norm is based on 50 times as many observations as is a given test-case, and if the variability of norm and test is the same, then, when the P.E. of the norm is taken as zero, the P.E. of the difference is altered by only one *per cent.* Since the norm usually does involve many more observations than the case to be compared with it, its P.E. can frequently be neglected. But one must neglect intelligently.

This principle of the negligible quantity can often be used in another manner. The norm, as we have seen, involves both intra-individual and inter-individual modes of variability. In comparing one individual with another, we work with the intra-individual variability; in comparing a group with another group (pathological cases with normal, perhaps), we take the inter-individual mode; in comparing an individual with a general norm, we use the combined measure of variabilty. If, however, either mode of variability is small with respect to the other, then, in those cases where the combined measure is needed, we may neglect the smaller. Presumably—we are not given the necessary data to make sure—the inter-individual variation in Grabfield's pathological cases is so large that it obscures a relatively small intra-individual variation, which we may therefore disregard.

In the second place, the psychophysicist, when his computation requires a P.E. which he does not know, may sometimes reason by analogy. Suppose that, from known averages and P.E.'s, he has found two or three groups, the averages of which are different, but not significantly different,—a frequent case, since significant differences in a common measure are rare. And suppose that the P.E.'s of these groups are practically the same. Then, if he is given the average of still another group without its P.E., he may assume that it has the same P.E. as the rest and compute the probable correctness of its difference from any one of the rest. If on this assumption he finds a significant difference, it is possible that he has assumed analogy on insufficient grounds; he should suspend judgment. If, on the other hand, he arrives with

his assumed P.E. again at a difference that is not significant, he will probably conclude that this is simply another case of an apparent difference due, not to the heterogeneity of the material, but to normal variability.

A second point of disagreement between Fernberger and myself concerns the generality of the application of the concept of numerical difference. Comparison, it seems to me, is as broad as science itself; in scientific experiment we change conditions and note the correlated change in a phenomenon, and this change is essentially a difference. Such a difference, when quantitatively expressed with available measures of precision, may conveniently be stated as a probability of difference (probable correctness). But Fernberger says: "We are also interested in the *amount* of that difference. It is true, for example, that we are interested in learning that the savage is more sensitive than the civilized man, but we are also interested in learning that the savage is *twice* as sensitive." And, undoubtedly, we might compute this probability, and also the probability that the savage is one and a half times as sensitive, and one and a quarter times, and so on indefinitely. In any such case we should always have two figures to keep in mind: the amount of the difference and the probability of that amount. If the total fact could be expressed by some single number, such as the probable correctness of the difference, surely an economy of thinking would be effected.

Let us be concrete. Suppose—to take a case of Henri's—that the average reaction-time in a set of 100 experiments is 24 hundredths of a second with a mean variation of 5, and that the average of another set of 25 experiments is 26 with a mean variation of 8. Is the difference between these two averages significant or due to chance? Our conclusion depends upon the six numerical values which we have just given, and we can not think in terms of six different values at once. We may, however, get rid of separate statements of number of cases by writing the P.E.'s of the means, so that we now have but four numbers to keep in mind; $24 \pm .423$ and 26 ± 1.352. We often wish to relate two such pairs of values without knowing how to do it; hence we had better simplify again by computing the difference, which is 2, and its probable error, which is 1.417. Now with only two terms we are getting within thinkable compass; and Fernberger has in them an expression for the amount of the difference and its probability. But we may prefer to simplify still further. Suppose someone should ask us whether this difference of 2 ± 1.417 indicates greater or less homogeneity than a difference, say, of 5 ± 2.8; we probably could not say. As a final simplification we may, therefore, take the ratio of the difference to its P.E.—in this present case, 1.411. Now we have reduced a six-fold statement to a single value. We have not really lost the amount of the difference, because all that we ever had was a difference with a certain probable error upon it, and the probable correctness reflects both the amount of the difference and its P.E. After we have convinced ourselves that a given disparity is significant, we may wish again to take account of its amount; but in so doing we imply a prior interest in its probable correctness. We are, as I have said, "interested in differences *between* limens;" first in the probable correctness and then, sometimes, in the amount.[4]

[4] In general I believe that Fernberger and I are in accord. I did not oppose his recommendations. I sought merely to point out some further implications of his data and to suggest the method of dealing with them.

What I have called the probable correctness of a difference is the probability that a given difference will not be negative, that is to say, that there will be disparity in the same direction as in the observed measures.[5] I have advocated this as the simplest possible measure of disparity and as one that is easily computed.

The question has been raised as to the relation of this "probable correctness" to the "probability that a difference is not due to chance," for which Henri has published a formula.[6] Since the nature of Henri's formula is not easily understood, it may be well to take this opportunity to explain it.

FIG. 1

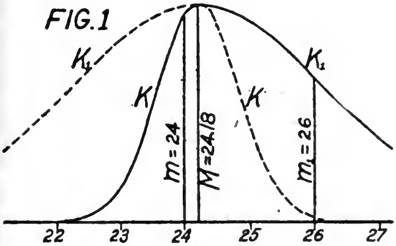
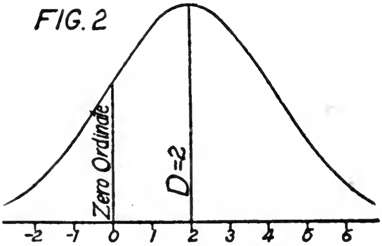


FIG. 2



Let us take the case of the reaction times (Henri, p. 159) mentioned above and let us construct a figure in explanation (Fig. 1). We have two means, $m = 24$ and $m_1 = 26$, which we may lay off on our abscissa. We assume tentatively that the two means represent homogeneous data. We now find a weighted average, M, between the two means, which depends on the measure of precision of each mean and the number of cases involved in each; Henri gives the formula (p. 155). $M = 24.18$. If the two means are really homogeneous, then this weighted average is the most probable value of the measure in question and should represent the median of a normal curve of distribution. We can draw such a curve about M if we know its measure of precision. If we regard m as a chance deviate from M, then we may determine the measure of precision with respect to which m, as a mean, varies (p. 156, top); and with this measure of precision we may draw the curve K, of which m becomes an ordinate. Now by means of formula

[5] It can be found, when we know the ratio of the difference to its P.E., from a table of the probability integral; *op. cit.*, 317. In the Cornell Laboratory we have a negative with the requisite equations and a graph of the probability integral, which make the finding of a probable correctness a simple matter. We can furnish blue prints from this negative at a very slight cost.

[6] V. Henri, Quelques applications du calcul des probabilités à la psychologie, *L'année psychol.*, 5, 1898, 153. I am indebted to Dr. L. T. Troland for pointing out the asymmetry of Henri's formula. It is in an attempt to meet certain difficulties that he raised that I add this discussion.

Henri's paper is both obscure and confused by inaccuracies. The following errata should be noted: (1) P. 156, formula (2): for "z_1", read "n_1". (2) P. 159, line 5: for "v", read "v_1". (3) P. 159, line 21: for "3.7", read "3.57". (4) P. 159, last two lines: for "$\frac{1}{89} = 0{,}01$", read "$\frac{100}{289} = 0{,}35$"; and for "0,02", read "0,22". The last error is a miscalculation which almost reverses a conclusion.

(2), (3), or (4) and a table of the probability integral, we find the area included between M and m. Thus, for $m = 24$, $t = .1988$ and the table shows that this area is 22% of the total area to the left of M. (Henri miscalculates this area as 2%!) This area is a measure of the amount that the mean, m, deviates from the theoretical mean, M, which, on the assumption that m and m_1 are homogeneous, is the most probable true value of the measure. But the farther m lies from M the less likely is it that m and m_1 (m_1 by being included in M is the cause of the disparity between m and M) are homogeneous. Thus Henri uses the area between m and M—22% in this case—as a measure of the probability that the difference is due to a determinate cause and not to chance. Conversely the probability that the difference is due to chance is 78%. Here Henri leaves us.

The difference that Henri has been measuring is not, as he implies, the difference between m and m_1; it is the difference between m and M. We can apply the same procedure to m_1 that we did to m. In this case we erect on M the curve K_1, of which the measure of precision is found from the data for m_1. K and K_1 will be different so long as the number of cases in m and m_1 or the measures of precision of m and m_1 are different. The probability that the difference between m_1 and M is due to a determinate cause is 64%. (Henri does not calculate this case.)

Thus it appears that the probability of an operative cause is 22% in the case of m and 64% in the case of m_1, when each is compared with the theoretically most probable mean, M. Can we get a single measure, as Henri does not, for the difference between m and m_1? If m and m_1 were really homogeneous, they would lie on the same curve and we could take the area between them. But, although Henri has considered the two as homogeneous, he has used different measures of precision for each of them, on the assumption (an error, surely) that the proper measure should depend solely on the one variant under consideration, and not upon both of the supposedly homogeneous variants. This mistake is theoretically fatal.[7] Nevertheless, if we want merely a practical measure, we may take the asymmetrical curve made up of K on the left and K_1 on the right, and (considering the areas on either side of M as equal) find the *per cent.* of the total area included between m and m_1. It is 43%. Such a value is not mathematically defensible, but it gives in practice results consistent with the values of "probable correctness," although smaller. It is the only way—so far as I can see—in which Henri's formula can be used in all cases to indicate the significance of a difference.

Fig. 2 shows the "probable correctness" of the difference in this same case of the reaction-times. The actual difference, D, is assumed to vary along a normal distribution curve, for which D is the most probable value, and for which the P.E. of D is the measure of variability. The probable correctness of the difference is the probability that D, in varying, will not assume a negative value, but will always represent a disparity in the same direction. This probable correctness

[7] On the other hand, some such incompatibility is essential to this mode of reasoning. When Henri assumed that m and m_1 were homogeneous, he assumed implicitly that one is just as often greater as less than the other,—the very sort of conclusion that his method aims to give. His formulation of the problem is fundamentally impossible; to think of two means as variants on a single curve is to deny that they have upon the curve fixed places, which would make it possible to determine a distance or an area between them.

is, therefore, the part of the total area under the curve which lies to the right of the zero-ordinate: in this case, 83%.

Since 50% represents pure chance, we may say that a probable correctness of 83% is .66 from pure chance to certain cause. The corresponding value that we got by extending Henri's method was .43. The reason for the discrepancy (.66 and .43) appears in a consideration of the two Figg. In Fig. 1 the difference m_1—m would be reversed if m_1 fell to the left of m or if m fell to the right of m_1. The chance of m_1 falling to the left of m is 39%; the chance of m falling to the right of m_1 is 18%. The chance of the difference being reversed in the one or the other of these ways is thus 57%, that is to say, the area not included between m and m_1. One is tempted to say, then, that the area between m and m_1 (43%) must give the probable correctness of the difference. This, however, is not the case, for m and m_1 ought to be taken as varying simultaneously. There would then be some cases of a reversed difference when both lay between their present values. Since the extension of Henri's formula omits these cases, the probable correctness (probability that the difference is not negative) must be a value greater than 43%. We have found it by the other method to be 66%, provided we take pure chance as zero, as we do in Henri's computations.[8]

It appears, then, that we may extend Henri's method to compute the probability of difference. This extended method and my own method indicate approximately the same thing; but Henri's method involves an unwarrantable mathematical assumption, it fails further to consider the two means as simultaneously variable (thereby giving values too low), and it requires approximately twice as many operations for the determination of a 'probable correctness.' What we want, I take it, is a formula that is both adequate and simple.

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[8] We can never, of course, come out with anything more than a probability. To ask whether or not a difference is significant is to talk common sense and mathematical nonsense. Mathematically we can indicate any degree of probable correctness between the limits of complete certainty and pure chance; and differences with various degrees of probable correctness are variously significant. A line between what is significant and what is not can be drawn only by some arbitrary convention. We may place it according to our own personal convictions or with respect to a consensus of scientific usage; or we may take into account considerations of symmetry, as Henri does, and call significant everything that is nearer certainty than pure chance. In this last case probable correctnesses over 75% would be designated as significant. Psychologists would probably put the division at 85 to 95%. The question is one for scientists and not for science, and it can never be permanently settled.

BOOK NOTES

Hindu mind training. By an ANGLO-SAXON MOTHER. With an introduction by S. M. Mitra. New York, Longmans, Green & Co., 1917. 536 p.

The author's interest in this field dates back twenty years when the problem of her eldest son's education began to take practical shape. Since then she has looked into many Western systems, Rousseau, Herbart, Pestalozzi, Montessori, and found none of them satisfactory. Later she came in contact with the Hindu system of education, under the guidance of S. M. Mitra, and this impressed her as on the whole better than any other, and the volume is an attempt to describe this system in a concrete way. The method does not require much of any gradation for it is a method of stories which illustrate virtues or great central themes of human interest. The idea is to select from Hindu literature a story of high illustrative value, tell, and then discuss it. For instance, the first story is that of a commercial genius, the theme being business sagacity. The second is true love or false, the theme being experience as a teacher. Then follow ill-gotten gains, choice or luck, diplomatic success, self-mastery, fortune, love conquers death, health versus wealth, wasted labor, wifely devotion, fatal inadaptability, the use of knowledge, prosperity and adversity, self-observance, psychoanalysis, and finally a long article by Mitra himself.

Man's unconscious conflict; a popular exposition of psychoanalysis. By WILFRID LAY. New York, Dodd, Mead & Co., 1917. 318 p.

Wilfrid Lay, who is said to be a very successful secondary teacher of much experience who had himself found help in psychoanalysis undertakes here to tell others what it means. To this end he discusses the unknown element in action, the Œdipus myth, the fore-conscious, the unconscious, descriptive unconscious, including symbolism, the censor, pleasure-pain versus reality, reality, regression, etc., the dynamic unconscious including craving reality, where thoughts come from, resistance, conflicts, complexes and phobias, and mental attitude, the individual psyche, dreams, two kinds of thinking (directed and undirected), every-day life, psychotherapy, and educational applications. The book is an interesting manual for the beginner and belongs perhaps in the class with books like White and Bjerre. The writer has a good insight into psychoanalysis and the first part of the book is remarkably lucid and popular, so that it would be almost impossible to miss its teaching. In the latter part he grows far less satisfactory. He betrays a good deal of interest in theories of the relation of the mind to the body, uses certain inapt phrases like "psychic gravitation." Some of his other attempts at modification or addition to the work of the experts we cannot pronounce happy.

The mythology of all races. Edited by LOUIS HERBERT GRAY. *Volume IX, Oceanic.* By ROLAND B. DIXON. 364 p. *Volume X, North American.* By HARTLEY BURR ALEXANDER. 325 p. Boston, Marshall Jones Co., 1916.

The writer of this notice has seen only a few volumes of this series. It is elaborately planned and has an able board of editors; the books

have an ideal, open and clean page, many illustrations, and form in general a good set of references and will prove not only convenient but indispensable for those who specialize in this topic. No single work, of course, can do justice to so vast a theme, and when we remember that Frazer alone has written over twenty volumes, we realize it would take a vast encyclopedia to cover all the ground. What is wanted is a finder that brings things out in true perspective and gives sufficient indication to literature to guide those who wish to specialize.

Our hidden forces; an experimental study of the psychic sciences. By ÉMILE BOIRAC. Trans. and edited with an introduction by W. de Kerlor. New York, Frederick A. Stokes Co., (c. 1917). 302 p.

The author is said to have "given psychical research its passport to travel freely on the road of scientific progress." A problem of mighty import is what has become of the souls of millions of men killed on the battle-field. The author makes a number of experiments in telepathy to show the conductability of psychic force, describes what he calls cryptoidal phenomena, concludes thought is the hidden force, would revise animal magnetism, describes the study of our superliminal powers as the psychology of the future. He is able to provoke sleep at a distance and to produce what he calls transposition of the senses, and describes even the colors of human magnetism. He calls the disconnection of motor nerve force exteriorization or human radiations, and tells us how to study spirits scientifically.

Children's association frequency tables. By HERBERT WOODROW and FRANCES LOWELL. Psychological Monographs, vol. xxii, no. 5, 1916. 110 p.

This is an attempt to make a comparative study of the mental associations of children and adults. It is a study of the psychology of these two groups, made by means of association frequency tables from the responses of a thousand children from nine to twelve, to each of one hundred stimulus words, viz., those of Kent and Rosanoff, and therefore they can be compared with their adult frequency tables, so that conclusions can be drawn from a total of two thousand cases of association. A secondary object of this study was attained by comparing these tables with children to secure data that may serve as a standard in the studies of mental associations of individual children. This method might be used for comparing children of different schools. This study shows that in general children's associations differ enormously from those of adults, and the frequency of various types of association. Adults excel in contrast, superordination, part-whole, noun-abstract attribute, participles and cause-effect. Children excel in verbs, verb-object, noun-adjective, adjective-noun, pronoun, sound simularity, contiguity, whole-part, subordination and word compounding. With only 39% of the stimulus words is the most frequent response the same for both, and with only five words are the three most frequent responses the same. The frequency of the favorite or most frequent response is about the same for both groups. Children give fewer individual responses. They use less number of different words in responses to a given stimulus word. The adults give more of the children's response words than the children do of the adults'.

Standard method of testing juvenile mentality by the Binet-Simon scale with the original questions, pictures, and drawings. By NORBERT J. MELVILLE. Philadelphia, J. B. Lippincott Co. (c. 1917). 142 p.

This is a manual for examiners using the Binet-Simon scale of mental tests. It is especially valuable for its emphasis on a number of highly important points frequently neglected. No man has told us what tests to begin with, or which of two alternative questions should be first employed, or under what condition a test may be repeated, etc. In part two we have such a uniform method of tests worked out in detail.

Two studies in mental tests. I. Variable factors in the Binet tests. II. The diagnostic value of some mental tests. By CARL C. BRIGHAM. (Psychological Monographs, vol. xxiv, no. 1, 1917.) 254 p.

We have here a new and clever discussion of various mental tests, based on Binet, with special reference to their diagnostic values. The author seems very chary about drawing any conclusions, and is dissatisfied with the general disagreement and inconclusiveness of this work.

A scale of performance tests. By RUDOLF PINTNER and DONALD G. PATERSON. New York, D. Appleton & Co., 1917. 218 p.

This book is an attempt to contribute to the few scales already in general use another kind of scale for the purpose of testing intelligence. The work grew directly out of the psychological examination of deaf children, for which purpose ordinary tests were practically useless. Hence these tests do not involve any kind of language response. The tests, fifteen in number, are first described, then their standardization, presentation of the data, the year scale, median mental age, point scale, percentile method, with illustrations at the close.

Mental adjustments. By FREDERICK LYMAN WELLS. New York, D. Appleton & Co., 1917. 331 p.

The author discusses in these eight chapters the following topics: mental adaptation; use and waste in thought and conduct; symbolic association; the continuity of emotion; types of dissociation; mechanisms in dissociated ideas; experimental appproaches; balancing factors.

Mental conflicts and misconduct. By WILLIAM HEALY. Boston, Little, Brown & Co., 1917. 330 p.

The author attempts here to give the living facts that show the great value of understanding the foundation of conduct and misconduct. Conflicts are sometimes accompanied by obsessive imagery, they sometimes cause impelling ideas, some arise from sex experiences or secret knowledge, others result in stealing, running away, they may touch parenthood. The work is very largely a set of case records, and other than this the author makes little attempt to add to our knowledge or to our theory. The reviewer should add that when the author speaks of analysis he does not mean psychoanalysis in anything like the technical sense, nor would it even appear that in discussing conflict he fully realized the broader basis that Freudian analysis has given to this general subject.

The psychology of special abilities and disabilities. By AUGUSTA F. BRONNER. Boston, Little, Brown & Co., 1917. 269 p.

With the increasing educational demands for recognition of the individual rather than of the mass, it is strange that no attempts have yet been made to formulate the problems of specialized abilities and dis-

abilities. Interest in defect has shed much light upon its different types. This author, however, here treats practical aspects, modes of attacking problem-cases, and presents types of special disabilities (a) in the normal, and (b) in the abnormal. Perhaps the best and most suggestive part of the book is the appendix, page 229, where the records and methods of psychological examination are given completely.

An introduction to social psychology. By CHARLES A. ELLWOOD. New York, D. Appleton & Co., 1917. 343 p.

The psychological study of social life designates by far the most important part of modern sociology, and the author here attempts to study the bearings of modern psychological theories upon the problems of social organization and evolution. Some of the more important chapters are: social psychology and its relations and methods; organic and social evolution; human nature and society; the nature of social unity; the nature of social continuity; social change under normal and abnormal conditions; instinct and intelligence in the social life; imitation, suggestion, sympathy and consciousness of kind in the social life; social order, progress, and finally the nature of society.

Wellesley College studies in psychology, no. 2. Edited by ELEANOR A. McC. GAMBLE. Psychological Monographs, vol. xxii, no. 4. October, 1916. 192 p.

This volume contains studies on the following topics: the qualitative relation between complementary and contrast colors; a study of spatial associations in learning and in recall; rate of repetition and tenacity of impression; the relative amounts of fatigue involved in memorizing by slow and by rapid repetition; a note on the use of the method of constants in experiments in intensive smell discrimination.

The adventure of death. By ROBERT W. MACKENNA. New York, G. P. Putnam's Sons, 1917. 197 p.

The purpose of this book is to show that death is not painful and that as a rule the most timid traveler divests himself of all fear when the shadow of the gateway looms over him. This is the conclusion of the author, who is a physician and is also that of his many colleagues with whom he has taught, as well as the available evidence of literature and history. It is especially appropriate in this war-time when death holds its high carnival. The author discusses not only the fear of death, its painlessness and euthanasia, and what it teaches life, but also in final chapters, whether death ends all. This question he answers in the negative if we rightly understand what death means.

A chemical sign of life. By SHIRO TASHIRO. Chicago, University of Chicago Press (c. 1917). 142 p.

This work attempts to apply facts discovered through the study of the physiology of the nerves to living processes generally, and the author thinks that in this mechanism culminates the most characteristic thing in life. He believes he has discovered its chemical accompaniment in nerve fibres. Despite its experimental basis, the work is highly speculative and the critical reader can hardly believe that he has discovered the true connection between irritability and metabolism, clever though his biometric work is.

The biology of twins (mammals). By HORATIO HACKETT NEWMAN. Chicago, University of Chicago Press (c. 1917). 185 p.

This volume brings together for the first time a mass of data on twins in man and other mammals. They are inherently interesting to many people and some light is shed on how twins "happen." The book ought to interest twins as well as biologists for the author discusses not only various kinds of human but animal twins. Perhaps especially interesting is the seventh chapter, on variations and heredity in twins.

The educational bearings of modern psychology. By CHRISTABEL M. MEREDITH. Boston, Houghton Mifflin Co., n. d. 143 p.

The topics are as follows: the nature of instincts; the modification of instincts (purposive action); the modification of instinct (mental growth); the growth of habits and sentiments; environment and suggestion; experiment in education; special studies in connection with memory; and special studies in connection with adolescence.

The spirit of the New Thought. Edited by HORATIO W. DRESSER. New York, T. Y. Crowell Co. (c. 1917). 297 p.

This is a volume of essays and addresses by representative authors in this subject: Henry Wood, Sarah Farmer, the editor, and some dozen others. New Thought seems to have become a kind of sect, in the interests of which these papers are republished. Together we believe that they give a pretty good idea of the modern phase of New Thought.

Cycles of personal belief. By WALDO EMERSON FORBES. Boston, Houghton Mifflin Co., 1917. 150 p.

The work is divided into four parts, namely, illusion, disillusion, reillusion and conclusion. The author discusses in general belief, the world, consciousness, axiomatic propositions, the will, ideas, time, law, unity, selection, and immortality.

How to develop your personality. By CLARE TREE MAJOR. New York, T. Y. Crowell Co. (c. 1916). 121 p.

This is a dramatic work dealing with various human qualities with reference to personality such as breathing, graded exercises, personality of the voice, its quality, what it betrays, poise, emotion, attitude to work, self-confidence, desires and ambitions. The author certainly treats her topics in a breezy way.

New Thought Christianized. By JAMES M. CAMPBELL. New York, T. Y. Crowell Co. (c. 1917). 152 p.

This is almost a kind of manual of twenty-one chapters.

The anti-prohibition manual; a summary of facts and figures dealing with prohibition. 1917. Cincinnati, Nat. Wholesale Liquor Dealers' Association of America. 121 p.

War and criminal anthropology. By ARTHUR MACDONALD. (Repr. from Congressional Record, Feb. 27 and March 15, 1917.) Washington, 1917. 40 p.

Success preparedness. By M. P. OLIVER. New York, T. Y. Crowell Co., (c. 1916). 103 p.

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A CHART OF THE PSYCHOMETRIC FUNCTION

By Edwin G. Boring

In a drill course in the psychophysical methods the instructor has often to face the problem of making the form of certain mathematical functions intelligible to students who have only the competence afforded by the ill-remembered contents of a high-school algebra. In the Cornell Laboratory we have utilized the principle of the 'coin problem'[1] to construct a chart, which (1) accounts for the form of the curve of error, (2) indicates the manner in which the *phi*-function of *gamma* depends upon the curve of error, and (3) shows, in the light of these derivations, why the Φ (γ)-hypothesis provides a plausible equation for the psychometric function,—all without the use of more mathematics than the simplest algebra. It is true that some arbitrary assumptions are necessary, but they do not, we believe, offset the pedagogical gain of simplicity. We reproduce the chart herewith. Its exposition is as follows:

Let us take as our typical problem the determination of the limen of dual impression upon the skin. The original formulation of this problem (Fechner) required that we discover what value of the stimulus in cm. (separation of the aesthesiometer points) is necessary just barely to elicit the impression 'two.' In practice, however, very different values of stimulus sometimes give rise to the impression 'two' and sometimes not, so that there is no fixed point of change. We may nevertheless conceive as fixed *at any given moment* a point of

[1] See, for example, L. D. Weld, *Theory of Errors and Least Squares,* 1916, 41-51. To the unmathematical psychologist, who would nevertheless become psychophysicist, this book can not be too highly recommended!

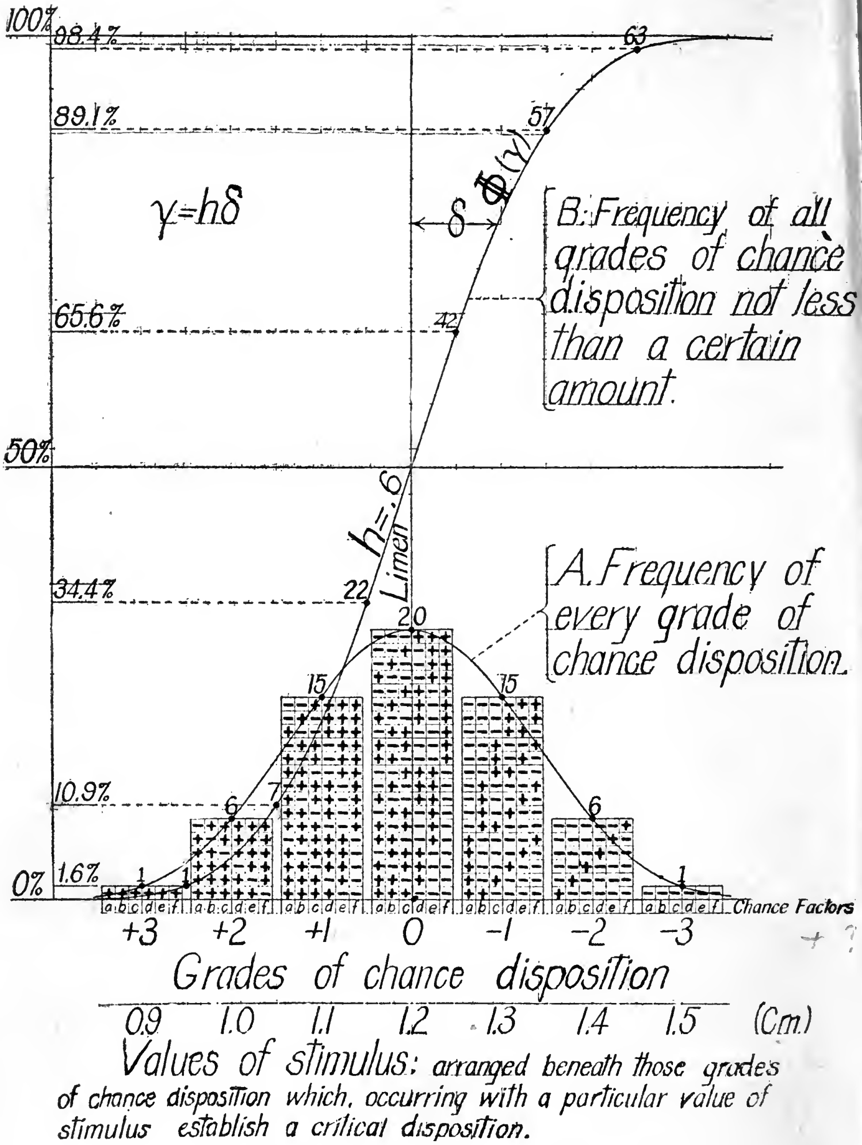

CHART OF THE PSYCHOMETRIC FUNCTION

change above which a stimulus would give the impression 'two' and below which it would not, provided that we do not forget the essential fact that this point is fixed only for the moment. The stimulated organ, we may then say, is variously disposed for impression,—a variation of disposition which occurs independently of the value of the stimulus, and as the result of the interplay of a large number of factors that work by chance, whenever the stimulus is applied, for or against the impression 'two.'

Ideally, in a psychophysical problem, we are concerned exclusively with a correlation between stimulus and impression. We presuppose an accurately controlled stimulus, an unvarying receptive apparatus the stimulation of which gives us the impression, and a mechanism that provides a report exactly reflecting this impression. Since, however, even in a careful experiment, we can control the stimulus only within the limitations of physical appliance, and the report only in so far as the observer can maintain constancy of attitude, we can never quite realize these ideal conditions. For the present, however, we shall overlook the possibility of departures from ideal conditions. We shall see, when we have finished our discussion, what part such departures may actually play in psychophysical work.

Let us suppose that there are, in connection with the organ of sense, just six factors, every one of which may dispose either toward or against the occurrence of the impression 'two' and therefore of the judgment 'two;' and let us call them *a, b, c, d, e,* and *f*. Further, let us represent a disposition toward the impression 'two' by "+" and a disposition against 'two' by "—". We assume that every factor disposes either toward or against the impression 'two' and never operates intermediately; and that the occurrence of "+" or "—" is independent of the stimulus and determined entirely by chance.[2] We have the possibility of seven grades of total disposition depending upon the chance occurrence of *pluses* and *minuses* for the six factors. (1) There is a single possibility that all six factors will dispose toward the impression 'two.' Let us call this grade of chance disposition "+3." It is indicated by the six concurrent *pluses* over the abscissa-value for "+3" on the chart. (2) There are six possible cases in every one of

[2]Actually we should need to assume an infinite number of factors or an infinite number of degrees of disposition; but we must keep our numbers finite or we shall complicate exposition.

If one wishes to think of "—" as the absence of a positive disposition rather than as a negative disposition, the argument is not altered.

which are five *pluses* and one *minus,*—a grade of chance disposition next less favorable to the 'two'-impression ("+2" on the chart). (3) There are fifteen possible ways in which four factors may dispose for and two against the dual impression, a chance disposition of "+1" (see chart). (4) The dispositional condition called "0" on the chart occurs when three factors dispose toward 'two' and three against; there are twenty such possible cases. When the *minuses* predominate, we get, in like manner, (5) fifteen cases for the negative disposition (against the impression 'two') of "—1," (6) six cases for "—2," and (7) a single case of maximal negative disposition, "—3." If we join these points we get the curve of error, which is marked "A" on the chart, and which gives the frequency of occurrence of every grade of chance disposition.[3] The familiar bell-shape of the curve represents the fact that there are more possible combinations of the chance factors when these are half and half unlike in effect than when they are all alike; and, further, that, in the region midway between these two conditions, the number of possible combinations decreases most rapidly.

The curve "B" is derived from "A". It shows the frequency of occurrence of all grades of chance disposition not less than (equal to or greater than) certain amounts. For instance, for the disposition called "+3" there is only the single case of the six *pluses* in which the stimulated organ is disposed for the impression 'two' by an amount not less than "+3." There are seven cases in which the dispositional grade is not less than "+2" (six in which it equals "+2" and one in which it is greater); there are 22 cases in which it is not less than "+1" (15 in which it is equal, 6 + 1 in which it is greater); and so on, with 42 cases (20 + 15 + 6 + 1) in which it is not less than "0," and 57, 63, and 64 cases in which it is not less than "—1," "—2," and "—3" respectively. Thus every point on the B-curve is the sum of all cases which lie to the left of it, as represented under the A-curve. The total number of cases is 64. If every sum is expressed as a percentage of the total number of cases we get the ordinate-scale at the left of the chart: 0, 1.6, 10.9, 34.4, 65.6, 89.1, 98.4, and 100%.

This B-curve is symmetrical about the 50%-line and would, if we had not arbitrarily taken a finite number of cases, be asymptotic to the 100% and 0% lines. It is known as the *phi*-function of *gamma,* Φ (γ). γ is a general unit of abscissa defined as the product $h\delta$, in which h is the measure of pre-

[3] Thus we may compute the ordinates by a formula for permutations; Weld, *op. cit.,* 42.

cision of both the A- and B-curves, and δ is the distance of any point on either curve from the ordinate of symmetry (in this case, the ordinate for balanced dispositional factors, the zero-ordinate on the chart).[4]

So far we have dealt only with the factors which vary by chance. The B-curve is merely an expression for the frequency with which dispositions toward the impression 'two', not less than certain amounts, occur by chance. But the controlled value of the stimulus also disposes more or less toward the impression 'two.' This impression will actually occur whenever the controlled disposition, dependent upon the value of the stimulus, and the total disposition of the receptive organ, dependent upon chance, are together adequate to the impression 'two.' The less the separation between the aesthesiometer-points, the greater must be the chance disposition of the organ, if the impression 'two' is actually to occur.

Suppose now that 1.2 cm. in the stimulus is critically adequate[5] to the impression 'two' whenever it occurs with the grade of chance disposition which we have called "0." Let us write "1.2" under "0" on the chart. A lesser stimulus, 1.1 cm., would require a greater chance disposition to be critically adequate to the impression 'two.' Let us say that 1.1 cm. is critically adequate when it occurs with the grade of chance disposition called "+1," and let us write "1.1" beneath "+1" on the chart. Then we may expect that 1.0 cm. will be critically adequate whenever it occurs with a chance disposition of "+2"; and so on. Thus we write under every grade of chance disposition the value of the stimulus which, in concurrence with that grade of chance disposition, would attain critical adequacy to the impression 'two.'

It now appears that we shall get the impression 'two' for a given stimulus whenever the grade of chance disposition which renders the given stimulus critically adequate to that impression occurs, or whenever there is a still more favorable grade of chance disposition. The B-curve shows the frequency of grades of chance disposition not less than given amounts; that is to say, the B-curve gives, when taken in relation to the scale of stimulus-values, the frequency with which a grade of chance disposition adequate to the impression

[4] For the use of these terms in the method of constant stimuli, *cf.* E. G. Boring, Urban's Tables and the Method of Constant Stimuli, *Amer. J. Psychol.*, 28, 1917, 281-285.

[5] By the phrase "critically adequate" we mean, in this discussion, to indicate the value which is the point of change from inadequacy to adequacy. We do not intend the Fechnerian notion; the point that is "critically adequate" is neither adequate nor inadequate, but is the point of change.

'two' will occur; or, more simply, the frequency of the 'two'-impressions for any value of stimulus. Thus, if we were to apply to the skin at random these values of the stimulus and were to record the frequency of occurrence of the judgment 'two' (which, we have assumed, corresponds exactly to the frequency of the impression 'two'), we should expect this curve of frequencies to be identical with the curve of dispositional chance factors (the B-curve), provided, of course, that our assumptions have been correct. The B-curve is, as we have seen, the $\Phi\ (\gamma)$; and a curve that gives frequencies of judgment as a function of the magnitude of stimulus is known as a *psychometric function*. Hence it follows that, under our assumptions, the $\Phi\ (\gamma)$ is the psychometric function.

We are now in a position to get some notion of what is meant by a *limen*. In practice, when we obtain empirically a set of data which approximate the $\Phi\ (\gamma)$ (the B-curve), we take the stimulus-value corresponding to the 50%-point of this curve as the limen. This is also the maximal point of the curve of error (the A-curve). The limen is, therefore, the value of stimulus which is critically adequate to the impression 'two' (hence the judgment 'two') whenever it occurs with the most frequent value of chance disposition; or, more simply, the limen is the stimulus which is most frequently critically adequate to the impression 'two.' Furthermore, we may note that the most frequent chance dispositional condition thus occurs when chance factors balance or cancel one another.

We began this discussion by making certain assumptions and we have argued that if these assumptions were valid the $\Phi\ (\gamma)$ would be the psychometric function. In practice, on the other hand, we find our psychometric functions empirically and then determine how closely they approximate the $\Phi\ (\gamma)$. We get, in practice, considerable agreement between hypothesis and fact; hence it appears that our assumptions are approximately valid.

Does this agreement between fact and theory also assure us that the value of the stimulus is accurately determined by the setting of the physical instrument and that the impression is exactly represented by the judgment? Not necessarily; it means that the errors which occur in either of these ways are relatively small, or are constant, or, perhaps, are due to chance. If small, they alter the limen by a negligible amount; if constant, they alter it by a fixed amount; if due to chance, they merge with the other chance factors and do not change the limen at all, although they may decrease its precision.

AN IMPROVED METHOD OF USING THE TELEGRAPHIC REACTION KEY

By HAROLD A. RICHMOND, Wesleyan University

As commonly used, the telegraphic key is not an entirely satisfactory reaction device. In the first place, there is the minor objection that it is not conveniently portable. It cannot be easily and quickly transferred for use in different places. If similar experimental conditions are maintained, the key must always be used at a constant height. Consequently, the table or stand on which it is placed must either be moved with the key, or a new stand of the same height must be provided for a different position of the subject. A more important objection is the fact that the release of the key may be brought about by a variety of possible forms of response. A mere lift of the finger will release the key, but the release may also be made by raising the hand, the forearm, the whole arm, or even by a movement of the body. From the use of the single pair of muscles involved in lifting the finger, the response may spread to the use of various combinations of large sets of muscles. As one set of muscles becomes relatively fatigued in making the reaction movement, another set may be brought into play. The importance of making the reaction movement as simple and uncomplicated as possible and of limiting it always to the same set of muscles needs hardly to be emphasized. Unless such a uniformity of conditions is obtained, the responses in successive reactions may be widely diverse in character. Under such circumstances we have no way of knowing whether a variation in reaction time is due to a change in the mode of response, or to a change in some other conditions influencing the reaction. One is not measuring homologous events.

Experimental analysis by Judd[1] and earlier work by W. G. Smith[2] has shown that the reaction movement with the ordinary use of the telegraphic key may be preceded by a sudden antagonistic movement, as well as various changes in pre-reaction muscular tension. Judd used a spring-supported key

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[1] *Yale Psychological Studies,* I, (N. S.), p. 141.
[2] Mind, XII, (N. S.), p. 47.

without arm rest. He found, however, that the antagonistic movements could be eliminated by requiring the finger to press upward against a second spring during the period of preparation. Since, as Judd's investigation shows, the reaction time is varied considerably by these antagonistic movements, they should if possible be eliminated.

The use of the telegraphic reaction key as developed in the Wesleyan Laboratory and here presented involves the suspension of the key, as a pendulum instead of on a spring, and employs the opposition of the thumb and fingers in producing the reaction movement. An ordinary telegraphic key mounted on a light wooden base is suspended so as to hang in front of the subject at a height at which the reactions can be most comfortably made. Generally, the same height as the elbow is the most satisfactory. The key is closed by placing the finger on the button and the thumb on the back of the wooden base, and pressing them together. By simply opening thumb and finger, the key is released. In point of view of its suspension and the employment of both thumb and finger, it resembles the modified form of the Dessoir reaction key devised by Scripture,[3] and has all the advantages of portability claimed for the latter. Instead of requiring thumb and finger to open in a vertical plane, as is the case with Scripture's key, the suspended telegraphic reaction key allows a horizontal motion of thumb and finger which is rather more natural and easier. Moreover, in Scripture's key the thumb is held in a relatively fixed position and plays no part in making the response movement. Furthermore, in making the response movement, the finger must move a mass and overcome a variable amount of friction. The suspended telegraphic key allows the thumb to move naturally and freely, and offers no resistance to be overcome by the finger.

With the use of the suspended telegraphic reaction key, the number of muscles which may be used in making the response movement is reduced to the fewest possible, viz., the set of muscles involved in the opposition of thumb and finger. By the use of these muscles and these alone can the key be released. Movements of the wrist, arm, or body cannot replace the reaction movement, nor retard it by an antagonistic movement. By thus confining the response to the action of a single set of muscles, one source of possible variations in reaction time is removed.

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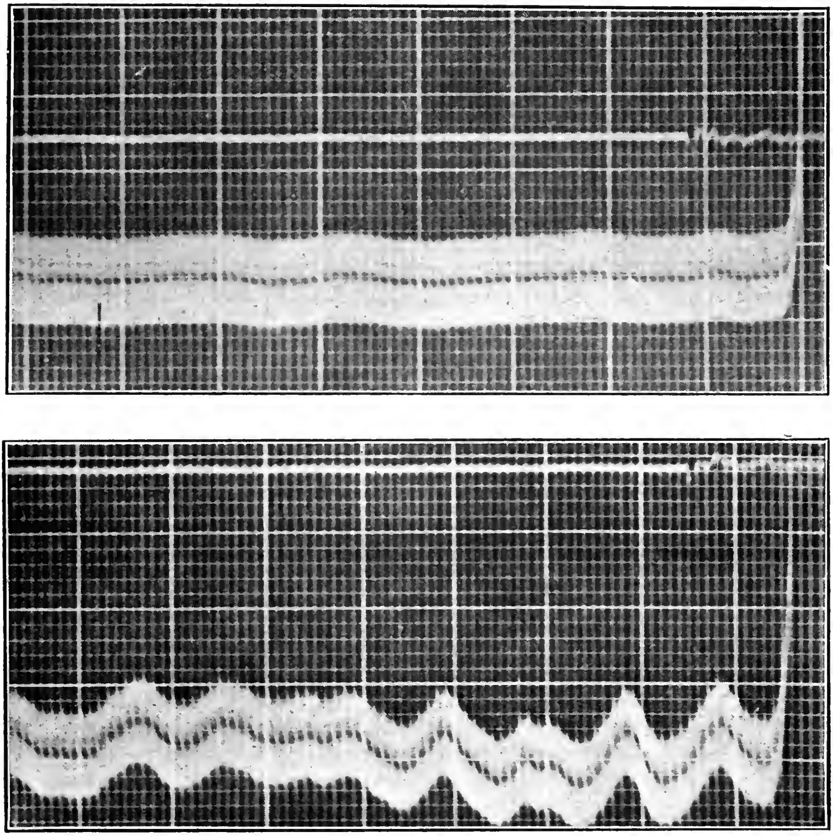
[3] *Yale Psychological Studies,* 1893, p. 88. See also Titchener, Experimental Psychology, 1905, II, Part I, p. 165.

FIG. 1



MICRO-PHOTOGRAPHIC RECORDS OF REACTION MOVEMENT SUPERPOSED ON MUSCLE TREMORS. (MAGNIFIED 250 TIMES.)

Another factor to be noted in connection with the use of the suspended telegraphic key is that it apparently eliminates the antagonistic reactive movements described by Judd. Enormously magnified graphic records of the reaction response gave no indication of such movements. In Figure 1 are shown reproductions of photographic records of the muscular changes in thumb and finger, before and during the response. These records were obtained by photographing the optically magnified movements of a fine silk fiber made to move in exact

correspondence with all rapid changes in muscular tension preceding the reaction movement. In taking these records, the regular telegraphic key was replaced by a small rubber bulb, which the subject pressed between thumb and finger and released in exactly the same way as he would the regular key. This bulb was connected by tubing to one end of a closed cylinder, the other end of which was covered by an elastic membrane, making a small Marey tambour. Any changes in the volume of the bulb with consequent changes of pressure in the cylinder would cause the membrane to move back and forth correspondingly. Attached to the membrane was the silk fiber, the movements of which, magnified 250 times by a projection microscope, were photographed by a falling plate camera.[4]

A sharp sound served as the stimulus to which the subject reacted, and the moment of giving the stimulus is shown by the break in the small line at the top of the record. The release of the bulb in the reaction response produced such a large movement of the fiber that it was carried completely off the plate, in spite of a vent to prevent rupture and eliminate slow pressure changes.

The device for transferring changes in pressure between thumb and finger to movements of the silk fiber was one of great delicacy. Relatively large movements of the fiber image resulted from very slight changes in pressure on the bulb. Muscle tremors and variations in muscular tension are conspicuous on the records. The records shown in Figure 1 are samples of two general types of records obtained in the experimental tests. In the upper record, the muscle tremors and changes in muscular tension during the preparatory interval are relatively slight; in the lower record, these variations are more emphasized. The different types were not limited to different subjects. From the same subject we obtained both smooth and wavy records in apparently chance succession.

Not one of our twenty odd records from four subjects shows any reactive antagonistic movement like those found by Judd. Judd finds a satisfactory explanation of the reactive antagonistic movements difficult, and ventures the opinion that they are due to a diffusion of the motor impulse which conditions the response. If such is the case, how are we to explain the disappearance of these movements when the subject reacts against the resistance of a spring, or by opening thumb and finger, as in the use of the suspended telegraphic key? A

[4] For a description of the falling plate camera see Dodge and Benedict, The Psychological Effects of Alcohol, 1915, p. 79.

possible explanation is that the sudden antagonistic movement is the result of a downward thrust of the finger to aid in the movement of the arm upward. Movement of the arm from the elbow means that the muscular contraction has to work against a long leverage. A slight downward push of the finger is certainly a great aid in getting the movement of the arm started, and is a natural mode of response. When, however, the finger is pressing against a resistance from above, any downward movement of the finger cannot aid in overcoming that resistance. Consequently, such a movement, being entirely useless, would be eliminated from the muscular response. It would be interesting to know whether those subjects in Judd's experiments who tended strongly to giving the antagonistic reactive movement did not also predominantly tend to use the arm in making the reaction movement. If such be the explanation of the antagonistic reactive movements as found by Judd, then it is not difficult to explain their disappearance in the use of the suspended telegraphic key, for this entirely eliminates the possibility of that form of response.

Furthermore, both mechanically and biologically, there seems to be no reason why the movement of opening the finger should be preceded by a movement in the opposite direction. But one set of muscles are involved in making the response, and it is hard to conceive how the contraction of a set of muscles would be aided by a preliminary contraction of their antagonistics. The upward movement of the arm, however, is directly aided by a preliminary downward thrust of the finger. Biologically, the opening of the thumb and finger is a natural movement, the movement of releasing something painful and injurious to the organism. It would be contrary to biological precedents if the movement of releasing an injurious object, such as a hot coal, for example, should be preceded by a movement to hold it more firmly. In any event, all the evidence at hand argues against the possibility of the reaction movement being preceded by a sudden antagonistic movement, in the use of the suspended telegraphic key.

The fact that the suspended telegraphic reaction key may be quickly and easily transferred for use wherever desired, requiring only some support for suspension, the fact that it limits the reaction movement to the action of a single set of muscles, and the fact that it eliminates an antagonistic reactive movement from the response combine to make its use an improvement over the reaction key as it is commonly employed.

SOME STRIKING ILLUSIONS OF MOVEMENT OF A SINGLE LIGHT ON MOUNTAINS

Joseph Peterson, University of Minnesota

It is now well known that our visual perception of any object in nature is determined not only by the rays of light from the objects that impinge upon the retina, but also by numerous other factors. Seeing is not a process of some "mind" looking out through the eyes upon the objects. The objects perceived are not so simply given in the objective world. Perception is quite a different process from such a passive procedure. This fact is more easily recognized in auditory than in visual perception. It is obvious that when we hear the robin outside in the spring we must be associating, though not consciously, visual factors with the sound, or how could we know that the sound is that of a bird of a certain color, size, and in a certain location? One also easily understands that such matters as the position of the head and the relative strength of the stimuli coming to the two ears are important in our determination of the direction of the source of the sound.

A large variety of factors has been shown to be of importance in visual perception. The story of the scientific advance in this field makes a chapter of real gain in the history of psychology. The location in the visual field of a dot on the wall, for instance, is determined in part by the position on the retina which it stimulates, or on which its image falls. If the dot is near the ceiling in front of one it stimulates the lower part of the retina; if it is at the right its image falls on the left portion of the retina; and so on. In consequence of this simple result of having a lens, the image on the retina of any perceived object, as is well known, is inverted. The old question of how the image becomes "turned right side up again" in getting to the cerebral cortex, or the "center of vision," is nonsense, and reveals a total misapprehension of the matter. No image is thus transmitted at all, but rather every part of the area of the retina stimulated is to be thought of as a particular case of the general law of the location of visual objects to which reference is made in

the early part of this paragraph. The tendency is to locate any object stimulating the retina, along the line determined by the point stimulated and the nodal point on the lens through which the ray passes. Since any point on an object stimulates a particular point on the retina, the various tendencies are co-ordinated in a way to give a total percept of the object —a certain shape, outline, volume, direction and distance consciousness.

The eye tends reflexly to turn so that the image of any stimulating dot falls on the fovea. Whether this tendency is wholly innate or is partly acquired, is not fully known. Or, better, how far the innate tendency is modified by experience in various animals and in man is a matter that is not settled. Certain innate reflex tendencies to eye movements of this kind are not to be doubted in some species of animals.

This suggests, too, that the eyes are not stationary with reference to any given position of the head, and the head is not determined by any given fixed position of the body. The direction of the visual object is therefore determined not only by the place of stimulation on the retina—for an object may be seen to move even though the eyes are fixed on it and are moving with it, in which case the image does not move across the retina—but also by numerous kinesthetic sensations. Normally various other factors are also important in the location of a moving object, such, for instance, as the images of other things over which the object is passing.

It is obvious, moreover, that all the diverging rays from any dot in the visual field must be brought to a focus on the retina, otherwise the effect would be that of a faint stimulation of the entire retina and the stimulations of several objects would all be superimposed and in no case distinguishable. This is the same thing as saying that the objects would appear blurred, or completely fused together. This focusing of all the diverging rays from any particular point in the visual field is effected by the refraction of the light rays as they pass through the lens. This requires careful adjustment of the lens for different distances, an adjustment that is brought about involuntarily by contractions or relaxations of the ciliary muscles. It must also be remembered that the perception is binocular. Therefore movements of co-ordination of the two eyes must play an important part. Any irregularities in any one of all these processes, due to fatigue, to unusual strain, or to pathological conditions must, of course, interfere with vision. Of such interferences resulting in illusions of different kinds and degrees Dr. H. A. Carr

has given and discussed a number of illustrations.[1] Illusions of depth have been known in some cases to be under voluntary control, as are also occasionally the clicks produced in the ears when swallowing, by the contractions of the tensor tympani muscles.

The perception of distance, or of depth, is dependent upon a large number of associated factors, such as color (since colors change with distance), clearness of outline, brightnesses and shadows, superposition of near objects on farther ones. Then, too, some more definitely kinesthetic factors, or impulses from muscular activity, play an important rôle: the apparent size of objects whose size is known, the relative size of various objects at different distances, and the apparent rate of movement of objects whose rate is fairly well known. A description of the rôle in perception of each of these factors would lead us too far from our special purpose. They are merely mentioned here to suggest the extreme complexity of visual perception.

Yet it must be emphatically stated here that perception is not a logical process, or even a matter of judgment, at all. We do not take note explicitly of all, and usually not of any, of these factors and then *conclude* that the object is located in such and such a direction at a distance of about thirty feet! This happens only when perception somehow misleads us. The whole process of perception is immediate. Instead of seeing in the distance a person known to be six feet high, noting that he appears very small, that some nearer objects conceal part of him as their outlines overlap his, that though walking he seems to move very slowly, that the outline of his figure is vague, that the strains of our ciliary and inner recti muscles are very slight, and so on, and then judging that he is so many blocks away, slightly to our right,—instead of such a deliberate process we immediately see him in a certain direction and at a recognized distance. The whole situation flashes into consciousness instantaneously, and does not come by any sort of inferential process whatever. This is what makes illusion not only so easy but so absolutely deceptive and stunning. We, of course, experience numerous minor and partial illusions which never are recognized as such. This is no doubt true with respect to any of the senses. It is really only when confusions and inconsistences arise in our experiences that our attention is drawn to certain imperfections in perception. Even in such cases analytic studies

[1] *Psychol. Rev. Mon.,* Vol. VII, No. 3; *Psychol. Rev.,* XIII., 258; *ibid.,* XV., 139; *ibid.,* XVI., 219. References to other writers are given.

in the psychological laboratories are necessary as a rule for a full understanding of the illusion.

The illusions now to be described are so interesting in themselves and cast so much light on certain larger problems of our orientation that it is thought worth while to report them, even though they were experienced so long ago that on many details where record is incomplete a high degree of accuracy in description is not possible. One of the illusions occurred in the writer's childhood and is purely a matter of memory. Only matters about which there is a high degree of certainty are given, however. The other illusion is a matter of record, but many of the details as to rate and direction of movement, the time for the illusion to develop, and so on, are lacking, some of the problems relating to which having been developed more recently in researches. It is hoped that the report, with such imperfections as it may have, may lead to the obtaining of more careful descriptions of similar illusions by those who chance to observe them.

On a dark rainy evening in December, 1909, from about 8 to 10 o'clock, a visual illusion of motion of striking proportions was witnessed by two high school students and four adults, two men of the latter being college students and the others being the writer and his wife. The illusion consisted in seeing movements of a stationary light on a mountain east of Provo, Utah. It first came to the attention of the writer and he called the other persons to witness the phenomenon. The part of the Wasatch Range on which the light was located is nearly directly east of the north side of the city. The observers were viewing the light from a position in the northeast part of the city, the house being located on a corner of Fourth East and Seventh North streets. The visible portion of the mountain on which the light was located has a height of about 1,000 meters (3,280.8 ft.) above the point from which the observation was made, and from this point an air line to the light, which was approximately half way up the mountain side, is about 2,000 meters, or something over a mile. The intense darkness of the night made only the outline of the mountain visible, the whole side from base to top being a perfectly dark field. This condition is well known to be most favorable for the illusion.

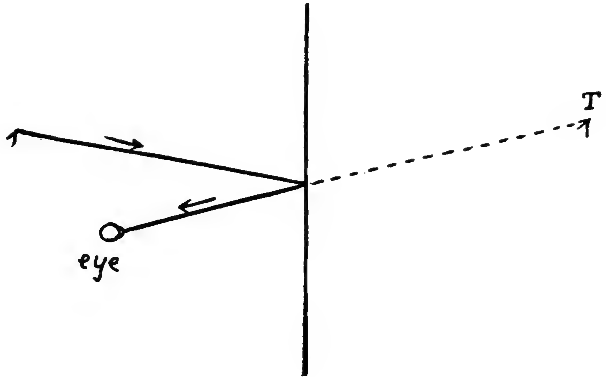
The writer watched the light some time himself before calling out the other observers. The phenomenon was really wonderful. One acquainted with mountain climbing knows that it is a slow, tedious process; yet here was a small, brilliant light, presenting much the appearance of a distant lan-

tern, moving about on the mountain side with an amazing speed. The light was, of course, larger than an ordinary lantern so far away would appear. The movements were very irregular, now and then presenting the appearance of one hesitating to think which direction to follow, *i. e.,* the movements were very much like the voluntary behavior of an individual rather than like some mechanical process. Frequently the light would, after such apparent deliberation, suddenly shoot out in a certain direction at a great speed. These extensive movements were always more or less up or down, frequently going obliquely to the sides in a direction making an angle not to exceed 30 or 40 degrees with a vertical line. Often it would seem to approach the upper edge of the mountain's outline, or, if going in the other direction, its base; but it, of course, regularly stopped and moved back. The backward movements were seldom, if ever, a direct retracing of the other path; *i. e.,* if we call the movements away from the true position of the light the out-movements and those bringing it back the in-movements, it could be said that seldom would the in-movements retrace even a part of the out-movements. The larger shooting movements were, moreover, not usually uniform in direction, but they were frequently subject to smaller irregularities. For instance, the light might shoot rapidly up slightly to the right and then turn somewhat at a particular place to this side or that; it might then pass downward in a more nearly perpendicular direction, and then up toward the original position. It should also be noted that occasionally movements up or down might go on for a short time without seeming to bring the light nearer the edge of the mountain; this, it seems to me, was particularly true when the attention was directed to the edge with a question as to how soon it would be reached. The light always stopped or changed direction before reaching the edge. Just how close it seemed to go I cannot say, because of the contradictory experience just noted.

When the other observers were called all agreed for a short time in their general descriptions of the movement. All seemed to get the illusion very readily, though possibly not immediately. We tried to sight by the edges of trees a few feet away, but in this we had no success. As the edges could not be clearly seen against the dark background afforded by the mountain, the sighting had no tendency whatever to annul the movements of the light. It is, however, possible that the projection of the trees above the image of the mountain, where they could be clearly seen, had the effect

of preventing extensive side movements of the light. Some of the observers were speculating as to whether some one might not be moving the light from a point not far away by means of strings. The high school girl, whose roommate was away, was frightened, regarding the phenomenon as supernatural! Those of us who suspected that the movements were illusory soon got confirmation of their view. Contradictions soon began to arise as descriptions passed beyond mere exclamation to greater accuracy. "There it goes, shooting upward to the right," *A* would say, only to be contradicted by *B,* who would declare that it was at the time actually moving slowly, or, perhaps, that it was going downward. It became evident that no two were having the same illusions.

It was then decided by the writer to arrange apparatus for accurate sighting, so that we could see what effect this would have on the illusion both during the time of sighting and



afterwards,—after the *fact* of the illusion had been clearly established wholly within the experience of each observer. Accordingly a dark east room in the house was chosen for an observation point. The light, it will be recalled, was to our east. In this room a head-rest was constructed a little over a meter from the window. From behind the observer's position, through a small space over and by the side of the door to an illuminated room a triangular ray of light was admitted. This ray was reflected from the window into the observer's eye, as shown in the accompanying figure, so that it would be seen outside of the window (at T) in the form of a triangle about four meters from the observer. The head-

rest was so adjusted that the light on the mountain was seen just above the point of the triangle. To any one of us as observer under these conditions the light on the mountain was seen to be absolutely stationary. Immediately, however, on removing his head from the head-rest and getting away from the controlled conditions of observation, the observer would again see the illusion as before. That is, the check of the experimental conditions had only a momentary effect on any observer: *the illusion persisted under normal conditions of observation even when it was understood, and had been shown by the observer's own experience, to be an illusion.* How much a considerable amount of practice in observation under the controlled conditions would in time affect the extent of the illusion was not determined. It is regrettable that under the rare conditions of the illusion at hand the effect of practice was not studied more fully. This may be done, however, in the laboratory. It has not yet been undertaken, I believe, though important studies have been published on the autokinetic illusion from the Chicago University laboratory since this illusion was perceived and partly written up.[2]

The light on the mountain was simply a camp fire by which some poor fellow was likely drying himself in the rain, wholly unconscious of the part he had played in our experiment. The writer has several times since the experience here described seen illusory movements of lights on mountains, but as, in very case, the lights were not so favorably located and the nights were not sufficiently dark, none of the illusions has been so marked. The movements as a rule have been but short, irregular fluctuations about the true point of the light, resembling more nearly the *Sternschwanken* of the early astronomers.

An illusion of the type here described, which was seen in the writer's childhood, is on the whole still clear in memory. Altogether three or four children and two adults witnessed the phenomenon, which was at the time wholly incomprehensible to the observers. It was regarded as something supernatural. A town about five miles from the one in which we were living at the time had just been, or was being, terribly ravaged by diphtheria. One family in two attacks of the disease had lost seven children, it was reported, leaving only the parents. The scourge had touched the more remote parts of our own town, taking a toll of three children from one

[2] Carr, H. A., "The Autokinetic Sensation," *Psych. Rev.*, XVII, 1910, 42. Adams, H. F., "Autokinetic Sensations," *Psych. Rev. Mon.*, No. 59, 1912.

family (including the writer's playmate) and one from another family. Stories were current of the dreadfulness of the disease and of how the dead were buried in the night. Beyond this town about seven miles was a mountain range possibly 800 meters in height, affording a favorable field for the illusion. This range, which our line of vision met at right angles, extended many miles both to the right and to the left. The illusion was witnessed on a dark night, and the observers watched it a long time with more interest than that of mere curiosity. It consisted of a large light moving horizontally from side to side over the unfortunate town. We considered it to be some sort of omen having relation to the scourge. Our descriptions, so far as memory serves me, were not specific enough to lead to obvious contradictions; and no one was in a mood to challenge any slight discrepancy. There was, however, some discussion as to whether the light did not have a short tail trailing behind it. I remember distinctly that its movements seemed to be more or less arbitrary, presenting occasional hesitancy and again sweeping at a nearly uniform rate for an apparent distance of one to two or three miles over the town. These long movements do not now seem to have taken more than a few seconds. They were not uniform in length and, it would seem, the light occasionally made actually less progress than the rate of movement would lead one to expect. The light moved only laterally, never up or down, as is now easily comprehensible since the dark field (the distant mountain) was narrow vertically and extended laterally.

Obviously this phenomenon was an autokinetic illusion. The light, which appeared very large even at the distance stated, was doubtless a fire on the mountain about twelve miles from the observers. Because of the state of mind of the persons witnessing the phenomenon, their interest in the neighboring town, the light was erroneously perceived to be the same distance away as the town. Normally in spatial perception our focus is on the object which receives the attention.

Of late, as has already been said, these illusions of movement of lights in dark fields, and of other objects presenting similar movements, have been subjected to careful study in the psychological laboratory. The phenomenon has been described as the autokinetic sensation, or autokinetic illusion. Various explanations of the illusion have been offered. One hypothesis has vaguely attributed the phenomenon to some unknown central factor—a very convenient way of disposing of the matter so long as no real explanation is at hand! Exner

has attributed the movements to overflows of the retinal excitation into portions of the retina near those stimulated. Three kinds of theories have been suggested which attribute the illusion to some sort of eye movements or tendencies to such movements, or to impulses commonly associated with eye movements. It is now well known that small, unconscious eye movements are constantly taking place. These small, quick movements, it has been suggested, may be fused unconsciously in such a manner as to give the impression of a continuous movement of the light in any one direction, or the shifting retinal impressions due to such movements may fuse together with the same general result. But it is difficult to understand how these movements could become integrated to produce the different gross effects experienced.

The second eye-movement theory attributes the illusion to actual eye movements of a considerable extent. Carr has shown, however, that after-images developed previously to the illusion did not bear out this theory; also that the illusion takes place without such eye movements. Illusory movements to the extent of 60 degrees or more may take place with a constant fixation.

The third eye-movement theory—which is, however, in reality an eye-strain theory—holds that the illusion is due to some sort of strain or fatigue in some of the eye muscles, producing an effect of lack of balance which somehow affects one's consciousness or perception of the direction of the light. This theory seems now to be the promising one. I quote a paragraph from a recent supporter of this theory:

"When the eye is at any given position in the visual field, it is held there by the balanced action of the six extrinsic eye muscles. These muscles are not all of equal strength and some will become fatigued more quickly than the rest. When they are in this condition, it will take more of an effort, usually reflex in character, to hold the fixation constant; the fatigued muscle will be more strongly innervated. Consequently, more impulses, kinaesthetic and others, will be sent to the higher centers from that muscle. As these impulses have previously been associated with eye movements in a given direction, and since the fixation remains with the light, the subject will think that the light has moved in the direction in which the strain has been exerted. It produces the illusion of a pursuit movement. It is a matter of indifference whether the nerve impulses from the muscles arouse consciousness immediately or not, so long as they are capable of overflowing into other centers and thus produce the consciousness of movement. The movement must also be attributed to the light and not to the ideational space, for if it were attributed to the ideational space, the movement would be in the direction opposite to that of the strain, whereas it actually is in the direction of the strain." [3]

[3] Adams, *op. cit.*, page 27.

Several experiments seem to support this theory and there are no serious objections to it. It is also obvious that some of the factors pointed out by other eye-movement theories may operate in some aspects of the autokinetic illusions, such, for instance, as are exhibited in the *Sternschwanken.* It must be remembered that perception is a very complex affair physiologically; that the visual perception of direction of any object cannot normally be isolated from that of all other objects in the visual field. When, therefore, the field is dark and presents only a single small light the visual checks by the various other objects are lacking and the illusion is apt to take place. It might be remarked that with only one such single experience there could be no direction sense at all, direction consciousness being—at least in its physiological aspects—wholly a relational affair. Even with the single light, direction must have meaning only in relation to certain marginal experiences or objects (tactual, kinesthetic, visual) and to certain bodily positions, which maintain for the time the general consciousness of the extension and location of the visual field in which the light seems to move. This is amply shown in illusions of direction orientation.[4] As a result of the formation of wrong visual associations during one's first experiences in a strange city or country, one may be "turned around" ever after in spite of efforts to correct the illusion. One may learn what the true north is and rationally orient one's self, but the *perception* still remains illusory.

It would be interesting to enquire into the frequency of the autokinetic illusion in the normal, or non-laboratory, experiences of man. The well-known *ignis fatuus,* popularly known as the *Will-o'-the-wisp* or the *Jack-a-lantern,* is likely due in large part, so far as its movements are concerned, to this illusion. It is a light that sometimes appears over marshy grounds, caused by the combustion of a gas formed by the decomposition of organic matter, the "marsh gas" or methane (CH_4). The conditions of the appearance of this phenomenon on dark nights (a small light in a large dark field) are entirely favorable for the illusion. It appears from reports of this "marsh light" that its movements are considerable. While some of the descriptions are undoubtedly exaggerated, it must be noted that a movement of 50 to 60 degrees, which under favorable conditions is entirely within the range of possibility, would seem to be extensive.

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[4] Peterson, J., "Illusions of Direction Orientation," *Jour. Phil., Psychol. and Scientific Methods,* XIII, 1916, 225. Other references are there given.

THE BIOLOGICAL SIGNIFICANCE OF THE EYE APPENDAGES OF ORGANISMS

By P. F. Swindle

I. Introduction

The following numbered statements include some of the more important observations which were made use of in formulating the system of thinking submitted in this article. Every outgrowth on the organism which is an obstruction to vision, will be considered an eye appendage; and any other object which serves the same purpose, will be considered a substitute for an eye appendage. For the sake of simplicity, only one or two kinds of animals are referred to in each case. It must always be borne in mind that some of the statements (1-14) are only very general ones; a number of exceptions can be noted in some cases, but a few of the statements scarcely need qualifying.

1) Nocturnal animals have as a rule white whiskers (cats).

2) Those of the diurnal animals are generally dark (squirrels).

3) Animals which are both nocturnal and diurnal have usually some white and some black whiskers; a single whisker may be for half its length black while the other half, the outer half, is white (rabbits).

4) Nocturnal animals which habitually nest in trees have some black and some white whiskers; the black ones project between the eyes and the sky, or the high objects in the trees, while the white whiskers lie between the eyes and the darker objects (in the night they are dark) the animal moves over (many opossums and raccoons).

5) Carnivorous animals possess, as a rule, the most pronounced whiskers.

6) While carnivorous animals lie in wait for prey they close the eyes seldom, but continually move the whiskers (cats owls, etc.).

7) Gregarious birds have usually no whiskers (partridges).

8) Whiskerless birds often nod the head quite periodically (doves, ducks, etc.).

9) Animals with oscelli and compound eyes usually possess prominent antennae which continually move before the eyes (the greater number of the invertebrates).

10) Those invertebrates which have the insect eyes and no prominent antennae, often raise and lower the body spasmodically or else move it from side to side (spiders).

11) As a rule creepers often thrust the tongue out of the mouth (snakes).

12) Many reptiles which lie quiet for long periods of time, often raise and lower the body or nod the head spasmodically (lizards).

13) Many aquatic animals which have no eye appendages frequently rock the body by lifting first one side and then the other (some salamanders such as Megalobatrachus maximus and Cryptobranchus alleghaniensis). Some cave forms have at most very degenerate eyes (Proteus anguinus).

14) Deep sea fishes often possess not only shaky telescoped appendicular eyes (Stylopthalmus paradoxus A. Br.), but some even carry luminous organs (they are supposed to be luminous) on pinnacles near the eyes (Melanocetes krechi A. Br.).

15) When a person carefully fixates with wide-open eyes a small object for only a few seconds, it subjectively disappears; but, as soon as the eyelashes are allowed to fall and for a very short interval of time obstruct the vision, the object reappears. Instead of dropping the lashes, one can cause the object to reappear by allowing a little smoke to pass between the object and the eyes. One can also move the eyes a few mm. to the side. Under any of these conditions the object reappears at once; but it does not necessarily retain its original color. If one carefully fixates a 10 x 10 cm. piece of green paper on a larger gray background, the color of the paper becomes induced on the background, and vice versa, to such an extent that all becomes one solid area of green; one can at times distinguish no transition from the paper to the background. However, if at this stage one lowers the eyelashes, the area of the colored paper becomes at once distinct, even though the color now observed is antagonistic to the previous

one. If one continues to thus move the eyelashes, or sprinkles dust particles over the paper, or spreads a tissue paper over it, or shifts the eyes a few mm. to the side occasionally, or closes the eyelids frequently while fixating, the form of the paper—whatever color it may from time to time assume—is easily retained.

16) If one fixates the green paper with half-closed eyes, it appears reddish after a very few seconds but retains its original form for a long period of time.

The function of eye appendages and their substitutes seems to be to retard the positive or self-induction and to hasten the negative induction of a color. An eye appendage or its substitute which serves this purpose, is a mechanism for training the visual apparatus to respond to colors in quite definite ways, as will be discussed in a later article. I wish now to do no more than merely mention that the organ of vision, including its appendages, is in truth a uniquely devised apparatus for training itself.

To hasten the negative induction of a given color, e. g. green, implies retarding its positive or self-induction, and from the biological standpoint this means of course no more than that a pause for green is hastily brought about. It is a matter of physiological necessity that a pause for green finally comes, simply because the nervous structures which are responsible for the sensation of greenishness cannot function indefinitely. After a recuperation pause for the structures involved, they can re-function and thus produce a second time the sensation of greenishness. If through some mechanical device the pause is hastily brought about, it need not be so long as it otherwise would be; green can be seen much earlier, and of course in a given time it can be seen much oftener. During the recuperation pause the organism is totally but only temporarily color blind for green. If an eye appendage brings about a pause for green before the exhaustion of the nervous structures for this color is very great, the organism will be able to respond sooner and in a given time oftener to the color presented it. The biological significance of this fact should be clear; it means that conditions are produced under which the organism is for no very great length of time blind to the particular color of the surface it fixates.

During the pause for green, I shall call it the 'green-pause,' the organism responds to a color which may be quite different from green; it is usually red, but it is of much greater biological significance that the area remains in one color or another than that it disappears entirely.

Each eyelash, each whisker, each condensed portion of a cloud of smoke, each dust particle, each translucent particle of a tissue paper etc., etc. serves as a background (or a foreground, if one should see fit to call it thus) however small it may be, on which a negative after-effect (in cases a positive after-effect) of the whole or a portion of a larger more distant object can fall either when the eyes move, when the background moves, or when the object behind the little background moves; each of these little backgrounds, whether it be an eye appendage or a substitute for one, produces a pause for a particular area of the retina that has neared exhaustion for green, e. g., but which can function strongly for the most unrelated color to green. Red appears as soon as this exhausted area for green (the exhaustion may be far from being complete) is no longer being directly stimulated by the green. When the nervous structures which terminate in the retina, become in a high degree exhausted for green, which occurs after many seconds of careful fixation, red will appear without the intervention of a background.

Any one of our little backgrounds serves exactly the same purpose for a limited portion of the retina as a large background does for the entire retina. The most natural background for the entire retina is the eyelid which occasionally falls, thus obstructing the vision entirely. If the small backgrounds are plentiful enough they also bring about a pause for the particular color of the fixated object, not necessarily for so many protected parts, but for the entire retina, i. e. for the unprotected areas as well; for the induced color, which makes its appearance under these conditions, can often not be said to be in any way different from that which appears when one closes the lids entirely. One great advantage in having numerous minute backgrounds instead of only one very large one is that the fixation can continue without the point of fixation shifting far or near or to the side; the organism is thereby enabled to see always the object it fixates, even though this object becomes subjectively differently colored.

II. Experiments, Observations, and Discussions

A. The Cat

But what would be the difference for a cat, provided the cat can discriminate between colors, if the mouse's nose it fixates should appear to the cat to be black, white, red, green, yellow or blue? A blue mouse would evidently be better than none. When a cat hears a noise in a pile of boards, it sits quietly near the boards and keeps its eyes turned in the direction of

the noise until the mouse makes its appearance, then the cat usually pounces onto it with one or both paws. If now a mouse comes to the end of a board and exposes only the nose, as mice generally do before scampering out into the open, the cat springs and captures the mouse by scraping it from behind the board with one paw which it can very expertly bend in a sharp angle to fit around the board. If the cat has no whiskers or eyelashes which it can continually move about in its field of vision as it fixates, it should theoretically be often unable to see the nose, because the nose, which is really a small point, should subjectively disappear. The cat must accordingly wait until the mouse shows a larger part of its body or comes entirely from behind the board and skips about. But mice very often show only the nose and turn back. The whiskerless cat would lose these opportunities, which are really good ones for a whiskered cat, and must necessarily live from the mice that venture into the open. Moreover, if a cat fixates an object before it begins to disappear and reappear, it cannot be blamed for growing restless as often as this occurs. And of course, if the cat moves about occasionally, this will disturb the mice. A smaller number of them would then leave their sheltered places to skip about the restless cat.

For many days I observed a cat while it was watching for mice. It evidently lived on mice and rats, for nobody fed it. During one afternoon I saw it catch two mice, and at dusk I found it with a rat. I deprived the cat of its whiskers and again observed it as it watched for mice. I saw it spring several times into the shucks and hay, but it captured no mouse while I was present.[1] It may of course be that I could not so well detect the presence of a mouse as even the whiskerless cat could, but it often seemed to me that the cat sprang when no mouse was about. I supposed that some of the objects which were in the cat's field of vision as it fixated, disappeared, and that when the animal moved its head or partly closed one or the other or both eyes, as it occasionally did, an object that had vanished suddenly reappeared and was mistaken for a mouse, perhaps not for the entire body but for such a small part as the nose.

The cat grew poor, each time it saw me it ran to me, rubbed for a while against my legs, and quite often looked me

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[1] I saw it one day with a snow bird that had just been captured, and a few days later I saw it eating a gray squirrel. The squirrel, however, had apparently died from wounds from a shot gun, for I found a number of No. 6 shot in its body. It had been dead so long that its eyes had shrunk.

in the face and meowed. As a whiskered cat it never did this. Instead of feeding it, I glued some rabbit whiskers at various places on its face, as much as possible onto the old whisker stubs. These would move about somewhat as the original whiskers did. For the sake of economy, I tried to give the cat movable whiskers; for a single whisker that moves in the field of vision is certainly worth a number of stationary ones. As long as the false whiskers were intact, the cat behaved just as it originally did; I never observed it spring without catching a mouse. But every time the animal washed its face, the rabbit whiskers would be either badly displaced or scraped entirely away. Since this method caused so much trouble, I shot some horned owls and glued many long shaky pieces of feather down before and behind the cat's eyes. Especially when the ends of these were charged with electricity, which I generated on the cat's back and transferred to the ends of the feathers, they stood out on the face and waved back and forth, in a way resembling the movements of the round arms of a sea anemone. When the cat now washed its face, the down was usually left intact, and all I had to do was to occasionally straighten and recharge the pieces.

I convinced myself that when the cat had false whiskers, it was a much more efficient mouser than when it had none. I saw it catch more mice, and it seemed to get somewhat fatter; but it still remained hungry only for the simple reason that I could not all the while be with it to replace the down it lost, to straighten the pieces that became tangled with the hair, and to keep the ends charged.

I finally captured in a trap another very wild cat which remained in the woods and fields. I shingled its whiskers and eyelashes and set it free. For many days it roamed about as usual, but after about two weeks I often saw it lurking about an old house which had been deserted for more than a year. I never succeeded in getting near enough to the animal to judge whether it was as fat as usual. Through inquiries I learned that the cat belonged to the last family that occupied the house, and that when the animal was deserted it began to roam in the fields and woods. I suppose that it, as a whiskerless cat, became less efficient in catching its prey, finally grew hungry, and returned every now and then to the house where it was formerly well fed.

B. The Squirrel

The squirrel comes onto a branch and carefully fixates another onto which it is preparing to spring. Since the whis-

kers are prominent on the animal's face and since a number of these are directly between the eyes and the branch it fixates, this branch may become white, red, green, blue or any other color (assuming that the squirrel can discriminate between colors). But whatever color it may subjectively become, it still remains for the squirrel a branch to which the animal can respond by springing. If however the branch subjectively disappears, as should occasionally be the case with a whiskerless squirrel, the animal might spring and fall to the ground. With these ideas in mind I captured some squirrels, deprived them of their whiskers, and occasionally set one free. I watched these carefully as they went running and springing through the trees. Very often a squirrel would be in the attitude of springing from the end of a limb of one tree to one of a neighboring tree and after all not spring, but start down the branch it was on, then run back up this branch, fixate the branch of the other tree for a short time, again run down the branch it was on, return again, etc., before it would finally spring. I was quite unable to observe this sort of behavior, which I observed in a limited number of cases, in any marked degree when I chased whiskered squirrels through the woods, or when I remained quiet and observed large colonies of wild squirrels often for four or five hours at a time.

Every one who has given particular attention to squirrels, knows how they jerk the bushy tail when they walk slowly or come to a stand-still. Since this moving tail falls in the animal's field of vision, I thought the hairs on it might possibly play an analogous rôle to that ordinarily played by the whiskers. I therefore sheared the tail of an already whiskerless squirrel and set the animal free. The number of fixations before springing was now unmistakably more, and once the squirrel missed its aim and fell about five feet to some lower branches; later it once fell to the ground.

It can of course be said that the purpose of the squirrel's tail is to guide the body as the animal springs. I have no doubt that this is the case; we might therein find an explanation for the fact that the tailless squirrel missed its branch and fell, but there are in this connection some very important facts to consider.

Following are a few auxiliary observations made on the human being. If one fixates a piece of green paper, e. g., for a very few seconds, and then drops the lashes for a very short distance, however not so far across the field of vision that they come between any part of the paper and the pupil, not only the top part but the whole of it changes immediately to red. So it

seems that since lashes or whiskers that move in any part of the field of vision during the period of fixation, cause the fixated object to remain more distinct, the hairs of a squirrel's tail, which move also in the field of vision, even though behind or above the fixating eyes, would play fundamentally the same rôle. Those hair-like processes, however, which lie directly between the pupil and the object of fixation, whether these be the whisker-like feathers on the sides of an owl's beak, whiskers on the nose of a cat, or lashes on the lids of the human eye, are the more efficient in serving the very important biological purpose of avoiding a subjective disappearance of the object fixated. Even the eyebrows of many human beings (especially among the American Indians) and those of such animals as the eagle, even though these processes come to lie, under conditions, only in the upper region of the field of vision, serve the same purpose. When the eagle or the Indian frowns, as is especially the case when a distant object is being carefully observed, the eyebrows fall and project a short distance into the field of vision just over the eyes; and careful observation shows that they are usually in a perfect tremor. I can observe this best on myself. (I am not a full breed Cherokee, but I nevertheless have many characteristics of this tribe). When I observe a distant object, I involuntarily frown; then the eyebrows nearest my nose project over and before my eyes and quiver. Moreover the skin, which ordinarily lies over the prominent bone projections above and toward the outer corner of each eye, falls at the same time against the eyelashes, thus shoving them over the pupil, though not entirely over it, but usually over only the upper part. Since this skin quivers rapidly, the eyelashes, which it is pushing down, also quiver. The eye of the Indian or of the eagle is, I think, a uniquely efficient apparatus for observing objects at great distances.

It does not matter whether the organism remains quiet as it fixates or whether it moves steadily about from place to place; it may soar in the air or walk steadily over a surface and still lose an object it fixates. When the surface is more or less homogeneous, such as is the case when a snow is on the ground, the fixated objects disappear more readily. While I was in the Ozarks, during the months of November, December, 1915, and the greater part of January 1916, hunting and performing the experiments already mentioned in this paper, I often found it necessary to walk a few miles to my hunting grounds. I followed the railroad track and learned to walk a rail for the entire distance without falling. Often after I had walked a mile or two, the track subjectively dis-

appeared, but returned again as soon as I puffed some smoke, not necessarily between the track and my eyes, but merely formed a smoke cloud at some place in the field of vision, or frowned, or reached my hand out before my eyes merely so that I could view it peripherally, or merely glanced at some distant object. At other times the track did not entirely disappear; when this was the case the parts between the ties remained quite normal while the other parts seemed to dip and pass under each tie. It was necessary for me to walk very steadily and at the same time fixate the track. My fixating the track as I walked steadily over it was analogous to my standing still and fixating a limited portion of it; under this condition it also disappeared for me. This was especially true at dusk, for the track, which was then for me much more like its surroundings, disappeared much more readily than in the day time.

When the rail apparently dipped under the ties, each tie was of course subjectively continuous where it was previously cut by the darker rail; each tie that crossed my field of vision induced itself across the darker rail. I observed in each case no more than an induced positive color (I use the Ladd-Franklin terminology and call everything colors excepting the psychological absolute black which I will define in a later article) of the tie and indeed an effect of long duration; for when I stood still I could often observe the induced color for a minute or more, i. e. until the entire tie disappeared, or, until I made a sudden movement of any kind, lowered the eyelashes, frowned, or did anything else of similar nature. In connection with this, I found it interesting to stand on the small railroad bridges (where the ties are much closer together than ordinarily) and fixate a tie and observe it as it suddenly flowed, as it were, to the neighboring ties, which in turn seemed to flow into the next ones, etc., until all became one homogeneous surface. I could easily avoid this illusion by throwing, among other things, the lashes over my eyes. It certainly did me no harm to have this illusion. If I had taken a step forward to walk to the other end of the bridge, the chances are that I would have stepped on a tie, for the simple reason that they were only about six inches apart; however, if my feet had been quite small I could possibly have stepped between two ties. If I had taken a step to walk to the other side of the bridge, i. e. across to the other rail, the chances for my falling would have been greater.

I can avoid the simultaneous positive induction of the color of the ties very easily by moving my hand in my field of peri-

pheral vision; I need not bring the hand between my eyes and the ties. It would seem that it is just as unnecessary for the squirrel's tail to fall directly between the animal's eyes and the branch it fixates. The jerking tail serves a distinct biological purpose even though it never falls directly between the eyes and the objects before the squirrel. We must furthermore consider that the squirrel often sits on its haunches and brings the tail up over its back. When it sits thus the tail is more directly in the field of vision, and this should really be the case; for the eyes are now in such a position that the squirrel can observe objects back of it almost as well as before it.

C. The Rabbit

The rabbit certainly needs no whiskers while running, for it does not move steadily over the earth, but leaps and bounds. Since the animal lies roughly entrenched, in a high degree unsheltered, during the day in open fields and with its head motionless and close to the ground, it needs black whiskers which project between the eyes and the sky, or in other words between the eyes and its diurnal enemies. Such whiskers are really present and they are constantly moving. The rabbit, which is by nature nocturnally inclined, is compelled by its many diurnal enemies to be more or less passively diurnal itself.

The rabbit would need no whiskers in the night time provided it never stopped running, but since it does stop and moreover since it often sits on its haunches, it really does need some. Furthermore, since it runs in the *evening* and at *night,* it needs some *white* ones. A white whisker on a dark background is of course more effective than a black whisker would be. When the animal is in the sitting posture, its head is quite differently inclined than while it is running or lying. The nose is now considerably higher than the eyes and consequently, owing to this position of the head, the white ends of some long half black and half white whiskers, which are rooted so low down under the nose that they serve the animal no purpose as it lies in its bed, now fall between the eyes and the dark objects on the earth. It is also interesting that the outer or white ends of these whiskers fall between the eyes and the more distant objects while the inner or black ends obstruct the vision for the closer ones. The black diurnal whiskers are thus converted into nocturnal ones by being thrown between the eyes and the horizon; and the white or nocturnal ones, which were of no use to the animal during the day, now lie in the most favorable possible position. This arrangement of the whiskers

of the sitting fixating rabbit enables it, among other things, to observe carefully its many nocturnal enemies. Besides its whiskers, its long moving ears also play a prominent rôle as eye appendages; the ears would seem to render a long bushy rabbit tail a nuisance as an eye appendage.

D. The Opossum

The opossum sleeps soundly during the day in secluded places, usually in tree cavities or in deserted nests of other arboreal animals. In the night time the animal walks or trots *steadily* over the earth. It should therefore have two sets of whiskers, some white ones which fall between the eyes and the dark objects it moves over, and some black ones which lie between the eyes and the higher objects which are surrounded by the light sky.

III. Conclusion

Paradoxical as it may sound, it seems that appendages on the body and substitutes for these which are really physical obstructions in vision, may after all have the very important biological significance of enabling the organism to retain objects it fixates. Since the carnivorous animals must necessarily fixate very often and for exceptionally long periods of time, it is not strange that they possess the most elaborate eye appendages. The herbivorous animals do not need to fixate so carefully the herbs they live on, but the significance of the occasionally highly developed eye appendages is nevertheless often to be understood when one carefully studies the activities of these animals. It is moreover not strange that animals which possess neither a great number of these appendages nor even a few moving ones, should very often spasmodically move the head or the entire body. Animals which live in flocks have a sufficient number of substitutes for eye appendages at their disposal. For any one of these animals, other members of the flock play the part of moving eye appendages. This is only one example of innumerable substitutes for eye appendages.

PRELIMINARY NOTE: THE INFLUENCE OF CHANGES OF ILLUMINATION UPON AFTER-IMAGES

By Leonard Thompson Troland, Harvard University

The conditions under which the quality and duration of after-images can be studied are so extremely varied that generalizations concerning the after-image process have little value in the absence of a detailed control and specification of these conditions. For this reason, I undertook, several years ago, the investigation of after-images produced under very special, but carefully controlled conditions, in the hope of obtaining consistent results. At an early stage in the research, I found that such results could only be secured when the effective aperture of the pupil was kept constant, because the character and life of an after-image depends not only on the momentary nature of the retinal stimulus, but upon the variations which this stimulus undergoes.

In all of the measurements to be considered in this paper, an artificial pupil, 2.36 mm. in diameter, and registered with respect to the line of sight by a device which I have described in another place, was employed. The after-image was produced by preëxposure of the rested retina of one eye to a semi-circular field, fixation being directed to a point in the center of the diametric boundary. The after-image was always observed by projection upon a complete circular field of the same quality as the semi-circular stimulus, fixation being maintained on the center of the circle, so that the conditions of observation were similar, in general, to those presented by a simple bipartite photometer field. In all cases, the surroundings of the test area were objectively dark, and the wave-length constitution and photometric brightness of the stimuli were carefully determined in absolute units.

Under these conditions, the mean variation of the time of decay of an after-image, for a single trained subject, is about 8%. It is not my intention to discuss here in detail the results of numerous measurements of the duration and intensity of after-images of this sort, made with a wide range of wave-lengths, and stationary intensities. It will suffice to say that

the duration is nearly independent of wave-length, and of absolute intensity, but varies radically with the diameter of the field and with the individual, as well as with the preëxposure time. Decrease of the stationary intensity below 10 photons (10 candles per square meter with a one square millimeter pupil) lowers the duration, and there is also a slight tendency for it to be lower in the red end of the spectrum than in the blue end. Increase of the preëxposure time above 100 seconds causes practically no increase in the duration. For myself, using a 1¼° foveal field, the maximal duration[1] varies from 70 seconds in the red to 95 in the violet.

In view of the slight influence of the magnitude of the fixed intensity, the remarkable effects produced by intensity variations in the reacting field, are of great interest. These effects may naturally be classified into two groups, those due to dimming and those due to brightening of the field. Hering and his pupils have made considerable use of dimming phenomena in their experiments to support the theory of antagonistic colors.[2] The recent very interesting studies of G. H. Miles—published since my results were obtained—on "The Formation of Projected Visual Images by Intermittent Retinal Stimulation"[3] undoubtedly bear upon the same fundamental processes.

If an after-image, formed and projected in the manner already described, be permitted to fade, and if then, the after-image of the projection field itself be observed against a completely dark ground, a demarcation of the field into halves can generally be noted. This effect is always obtained with short preëxposures and high intensities, but is absent for preëxposures which approach in duration the "equilibrium time," or maximal duration of the image.

If the intensity of the projection field is decreased, but not made zero, a similar but usually far more striking rejuvenation of the after-image contrast, occurs. The preëxposed half appears darker and more or less complementary in hue as compared with the other half of the field. In its general form, the principle that *dimming the reaction light enhances the processes which are "antagonistic" to the original stimulation,*

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[1] The duration increases asymptotically with increase in the preëxposure time. For a discussion of this law, see L. T. Troland, Apparent Brightness; Its Conditions and Properties. *Transactions of the Illuminating Engineering Society,* XI, 1916, 947-967.

[2] See *e. g.,* R. Dittler, and Satake, Eine Methode zur Bestimmung der gegenfarbig wirkenden Wellenlangen des Spektrums. *Zeitschrift für Sinnesphysiologie,* XLVIII., 1914, 240-252.

[3] British Journal of Psychology, VII., 1915, 420-434.

is familiar to all readers of Hering. However, in order to account for the increase of the after-image contrast, the principle must have a special mathematical form, since if dimming affected both halves of the field equally, the contrast would not be augmented, however great the absolute alteration of the compared areas. Consequently, it is necessary to state that a very slight difference in the level of fatigue of two visual areas furnishes the condition for a large difference in the magnitude of the *dimming effect* producible on these two respective areas. This is the principle of the *dimming contrast effect*.

An explanation of this somewhat extraordinary principle is suggested by further study of the phenomenon itself. The contrast, which has been reëstablished by dimming, fades quite rapidly on the dimmed field, but if—after it has disappeared—the field is brightened to its original intensity and then dimmed once more, the contrast returns; a process which can be repeated successfully over a period sometimes twelvefold the life of the image on the undimmed field. This factor of the relative *durability*[4] of the dimming contrast effect decreases rapidly to an asymptote with increase in the original preëxposure time; in other words, the effect is less marked in conjunction with a generally low level of visual sensitivity than it is with higher levels. Increase in the degree of dimming reveals a maximum of the "durability" of the effect in the neighborhood of one-half the primary intensity. From $100\,\sigma$ to $600\,\sigma$ the durability increases practically as a linear function of the time during which the dimmed intensity is maintained at each successive test. Between .7° and 3.5°, a linear relationship exists between the durability and the diameter of the stimulus field. When the primary intensity is varied, and the dimming is always to a constant fraction of this intensity, the effect shows a maximum in the neighborhood of 100 photons, depending on the value of the fraction. This law also applies to the maximal duration of the dimming contrast in the dimmed phase, if this is prolonged. The latter duration decreases asymptotically to zero in successive intermittent dimmings.

Certain of the above facts strongly suggest that the dimming contrast effect depends upon a difference in the rate of decay

[4] The "durability" is to be distinguished from the "duration" of the effect, the former term referring to the period during which it can be obtained by repeating the process of dimming (and brightening), the latter designating the life of the image in a single cycle of illumination change.

of some component of the excitation in the contrasted fields, this decay taking place more rapidly in the more fatigued area. In other words, the more fatigued area shows a lessened resistance to change in its state of excitation. The dimming contrast, on this theory, should be in evidence only *during* the change, and the fading of the contrast would indicate that the change had been completed in both halves of the field. On the other hand, if the change were repeated, by brightening and redimming the field, the contrast would reappear, but would persist a shorter time than before, because of the nearer approach of the resistance factors of the two halves, to equality. This explanation applies to both the chromatic and the achromatic aspects of the phenomenon.

The assumption that exposure of a visual area to stimulation decreases the resistance which it offers to change in its state of excitation is in harmony with the laws stated by Helmholtz for the duration of positive after-images,[5] and is also borne out by the remarkable effects produced by *brightening* of the field under the general experimental conditions above described.

When the field is brightened, in general the faded negative contrast is *reversed,* and becomes *positive.*. The preëxposed half appears brighter than the non-preëxposed half, and becomes more saturated than the latter, which tends to acquire a hue complementary to it. This *reversal effect,* or brightening contrast effect can be obtained either by brightening on the basis of the primary stimulus intensity, or by dimming, permitting the dimming contrast to fade, and then restoring the primary intensity. The positive contrast fades rapidly on the brightened field, but—as in the case of the dimming contrast—can be rejuvenated by repeating the cycle of operations. With a spectral red stimulus, the total durability of the reversal effect, seems to be of the same general magnitude as that of the dimming contrast effect, when the field is alternately dimmed and brightened with equal intervals. To obtain the reversal contrast at its maximum, it is necessary to permit the preceding dimming contrast to fade completely, and *vice versa.*

I have made a number of careful studies on the laws governing the duration of the reversal contrast after any single brightening, when the bright phase is prolonged. In these experiments, the reversal was obtained by first dimming the primary intensity and then brightening it again, after an interval. It was found that the duration of the contrast increased to a maximum as this interval was lengthened, and then

[5] Handbuch der physiologischen Optik, 3te Auflage, 2, 1911, 194-202.

decreased asymptotically to zero. A similar law was found to connect the duration with the dimmed intensity, a maximum occurring for a dimming to about 1/50th of the original value. Variation of the preëxposure time also produces a maximum, and an asymptotic fall with the higher preëxposures (under the conditions which I employed, above 20 seconds). Other things equal, the effect is much more marked on large than on small fields.

These correlations are consistent with the view that the reversal of the negative after-image caused by brightening of the projection field, also depends upon a lessened resistance to change in state of excitation, due to preëxposure. The reversal, like the dimming contrast, persists only during this change, but can be rejuvenated by repeating the cycle of operations. In other words, the two phenomena are obverse and reverse of the same underlying condition. I do not claim that there are no difficulties to be met by this view, but it offers the most satisfactory synthesis of the facts that I have yet been able to find.

One difficulty lies in the manner in which the two effects vary with the wave-length of the stimulus. I have thus far not made searching experiments on the influence of wave-length upon the dimming effect, but cursory observations with representative spectral colors have not shown any marked influence of this factor. The dependency of the reversal phenomenon upon wave-length, however, appears to be quite clear cut. When the intensity change employed has a 10:1 ratio, the reversal is obtained at very low intensities in the red end of the spectrum,[6] but as the stimulus moves towards the blue end, higher and higher primary intensities are required, and the effect is absent beyond the green under all conditions which I have tested systematically.

The colors and color contrasts generated in the dimming effect are extremely vivid and variegated. In general, as would be expected, dimming tends to move the apparent color of the field along the spectrum towards the complementary of the "real" color. The spectral distance moved through increases with increase in the fractional dimming, with increase in preëxposure, and with decrease in the primary intensity. The direction of movement[7] in the spectrum, as well as the distance, varies with the wave-length of the stimulus. From

[6] These experiments were carried out at the Nela Research Laboratory.

[7] Direction of movement is determined by use of a graded series of values of the governing variables.

the extreme red to the yellow the change occurs through the purples; between the yellow and yellow-green there is reversal of the direction; between green and blue-green a further reversal; between blue and violet a third change; and between violet and extreme red, again, a fourth shift in direction. These facts indicate that there are four points in the color circle, for which dimming would produce only a saturation change, unless it could suffice to produce the complementary. In all cases the color contrast fades far more rapidly than does the luminosity contrast. It will be observed that all of these relations are in harmony with Hering's general schema of the visual qualities.

Although certain aspects of the dimming and reversal phenomena are explained by Hering's assumptions, these assumptions are by no means sufficient. Moreover, general physiological objections to Hering's doctrine of sensory metabolism make it illegitimate to employ his hypotheses in the exact form in which he stated them. However, my attempt to modify his views[8] so as to meet these objections yields postulates still less adequate, so far as the dimming and reversal effects are concerned, although they satisfy the laws for fatigue with stationary illumination much better than do Hering's original conceptions. My present opinion is that the former effects depend either upon the laws governing certain of the "constants" or parameters of the simple equations for retinal excitation, or else that they rest upon conditions in the visual system posterior to the retina.

I have met with some success in an attempt to explain them as a result of *Bahnung,* or the reduction of synaptic resistance by excitation. On this basis, the rate of change of excitation in a synapse may be conceived to be proportional to a force factor, representing the intensity of the incoming current, and inversely proportional to a resistance factor, representing the susceptibility of the synapse to alteration of its state. This makes the excitation an exponential function of the time after any change in the force factor, and of the resistance. The resulting equations meet many aspects of the experimental situation in a satisfactory qualitative way. However, at present, I regard this theory as highly speculative.

In connection with the above, I wish to mention several phenomena upon which thus far, I have made no detailed quantitative studies. In the first place, by use of the dimming procedure, after-image contrasts can be obtained which endure

[8] L. T. Troland, Adaptation and the Chemical Theory of Sensory Response. *American Journal of Psychology,* XXV., 1915, 500-527.

three or four minutes, on the basis of only an eighth of a second difference of exposure of the two halves of the field. I have been unable to secure these contrasts with red light, but they are easily produced with blue. Under these conditions, positive contrasts, or reversals, are sometimes obtained during the dim phase. Another interesting effect is the spontaneous appearance of a reversal, immediately following the disappearance of an ordinary negative contrast (through fading), without alteration in the intensity of the field. This effect is obtainable with all of the spectral colors, but most easily with the end-regions of the spectrum, and with short preëxposures. It is also possible to produce a positive image as an initial result of preëxposure, when the blue end of the spectrum (especially the blue-green) is employed and the preëxposures are less than 16 seconds.

VALUE *VS.* TRUTH AS THE CRITERION IN THE TEACHING OF COLLEGE PHILOSOPHY

By Wesley Raymond Wells, Washington University

The teacher of philosophy tends to be so exclusively a system-builder, with an eye single to the logical construction of his philosophy, that the concrete, human needs of his students are apt to be lost sight of. In general educational theory there is taking place a reform of method, and a reaction against the mere teaching of subject-matter regardless of the interests, aptitudes, and needs of the pupils. Teachers formerly taught subjects of study; now they teach children. The lesson learned from the elementary schools, that teaching should be ordered to fit the psychological needs of the pupils rather than the logical claims of the subject-matter, may with profit be extended to the teaching of philosophy to college undergraduates.

The vast majority of individuals never become students of metaphysics in any technical sense of the term. Of those who study philosophy in college, many simply "take courses", for sundry reasons, without ever acquiring much interest in the subject. With a large number of the students who really become interested in philosophy, religious doubt is the beginning of a philosophical interest that arises in the attempt to bolster up a waning faith. Metaphysical interests arise in others as a direct outgrowth of the developing sex life in early adolescence, as in the case of Don Juan, who—

> "Did the best he could
> With things not very subject to control,
> And turned, without perceiving his condition,
> Like Coleridge, into a metaphysician.
>
> "He thought about himself, and the whole earth,
> Of man the wonderful, and of the stars,
> And how the deuce they ever could have birth."[1]

As the poet says, with true Freudian insight, "puberty assisted" in bringing about Don Juan's philosophical interests.

Mere intellectual curiosity, or the pure love of wisdom for its own sake, is a possible though negligible cause of a

[1] Byron: "Don Juan," xci, xcii.

student's interest in philosophy, at least at the beginning of his studies. As a general rule, philosophy is valued at first only as an aid in solving the fundamental problems of life, and not as an end in itself. With prolonged study, perhaps unfortunately, philosophy may become an end in itself, divorced from all practical problems of life, through a psychological process like that involved in the case of the man who took his first drink to save his life, and thereafter lived to drink.

"Longings sublime and aspirations high" come naturally enough during adolescence, and the instruction of college undergraduates should have due regard to the moral needs of the students. College students of philosophy desire little philosophic dogmatizing from instructors, but, on the contrary, a free field in which to draw their own conclusions. The trend of their speculations can be guided, however, if instruction is insinuated gently, and not applied bluntly. Since metaphysics is a subject on which the most learned of doctors disagree violently, disciples are free, to a large extent, to choose metaphysical conclusions,—such, at least, as are really relevant to human concerns, upon the basis of value, regardless of truth. The question thus presents itself as to the sort of philosophic beliefs that are most valuable for students. This problem is similar to the problem of the value of religious beliefs.

Whatever may be the latest philosophic conclusions of a teacher of metaphysics, they are usually not the same as his earliest conclusions. The failure of a man's metaphysical views to broaden and grow with continued study would be an indication of intellectual stagnation. In every special science, at least some few results are established, and accepted by all scientists without question. Such results have become fixed in the elementary text-books. But metaphysics is a different matter. There is no generally accepted text-book of metaphysics. When it is the case that the teacher of philosophy has come to have somewhat different philosophic views from the ones that he held when he was a younger student, as is the normal case, he is very likely to infer from this, if his attention is called to the matter, that his own students will not at first acquire philosophic truths in final form. Even if there be final and absolute Truth in metaphysics, the human acquisition of this Truth is a psychological process, always incomplete and always imperfect. The teacher of philosophy, as a teacher, should be primarily concerned, not with absolute Truth, its existence being granted for the sake of argument, but with the psychological learning processes of his students in

their acceptance of some views and rejection of others, for emotional as well as for logical reasons. The student believes that his conclusions are reached by purely intellectual processes, but the educational psychologist recognizes that the passional nature is a large factor in the process.

Since human nature is such as it is, idealistic systems of philosophy, whether true or not, will always appeal to students of philosophy, and will be accepted by many for the reason that man's emotional nature, in so many cases, requires some sort of idealistic beliefs about reality. As James says, man's "will to believe" will assert itself in a large percentage of cases, and will accept an idealistic view of things because such a view is congruent with certain vital needs. Whether or not many of the current systems of philosophy that are called idealistic really offer support to man's specific religious beliefs,—even the most general and fundamental of them, is a question that need not be raised. They at least *seem* to most students to do so,—to offer a refuge of respectable supernaturalism against the encroachments of scientific naturalism. Upon the basis of value, and regardless of objective truth, the teaching of the great historical systems of idealism is certainly justifiable.[2] To force upon students a completely naturalistic philosophy, even if such were the true philosophy, —a philosophy that proclaims the universe to be essentially indifferent to the ideals of man, would be extremely disintegrating, especially for those adolescents who were glowing with the enthusiasm of new moral aspirations. Naturalistic beliefs would crush out the incentive to noble effort. But belief in idealistic philosophies, with their appearance of devoutness, however vague, lends grandeur to the universe, and zest to the moral urgings of the individual life. Several students of my acquaintance, now graduate students in philosophy and in other departments at Harvard, have told me that, while not now calling themselves metaphysical idealists, they nevertheless gained moral support and stability of character through studying, when undergraduates, such sympathetic treatments of idealism in its modern historical forms as are found in some of the chapters of Royce's Spirit of Modern Philosophy. This is, I think, a not uncommon experience. It is reported[3] that Jowett encouraged the study of the philosophy of T. H. Green by Balliol undergraduates because of its religious value for the students who were in search of a substitute for their earlier religious beliefs.

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[2] *Cf.* G. Stanley Hall: Adolescence, vol. ii, p. 551.
Cf. F. C. S. Schiller: Studies in Humanism, pp. 278, 79.

Moral idealism is *theoretically* independent of any particular religion or system of metaphysics. It can be established to the full satisfaction of the intellect along with a naturalistic metaphysic as well as with an idealistic one. But *practically*, for actual human beings, most of whom, fortunately, are very incompletely intellectualized and de-emotionalized, theoretical moral idealism can become embodied in actual conduct only through the instrumentality of religious and idealistic beliefs. Belief in God and immortality, for example, tends to moralize the whole life, and to support such forms of conduct as are judged by the theorist to be right. The ethical theorist is concerned with discovering *what is right*. The preacher is concerned with influencing the will of man *to do the right*, that is, with making man's conduct conform to the standards set up by the theorist. Similarly the teacher of philosophy has a mission to perform in offering to youthful students such philosophic beliefs as will encourage, not discourage, moral effort on their part.

The educational psychologist would prescribe the early studies in philosophy upon the basis of their psychological adequacy, and their emotional congruity with the requirements of the moral life. A sympathetic presentation of the idealistic philosophies of history, with a greater effort at exposition than at criticism, has valuable results, while the exclusive teaching of naturalistic views would have disastrous moral effects. Perfect intellectual honesty and candor on the part of the instructor can be united with a due regard for the values of the metaphysical views that are incorporated in the various idealistic philosophies in history.

THE TERM REACTION TIME REDEFINED

By P. F. Swindle

For a particular simple or complex reaction, the cockatoo has at least as many reaction times as it is trained to have. By the term reaction time should be understood *the time which intervenes between the presentation of a stimulus and a response in which we are interested.* The natural reaction time to the stimulus green is too short for our modern instruments to measure; this is certainly the case with human beings, and we have no reason for assuming that it is any different with a bird. The most primary reaction to green is the immediate response to green, and not a certain movement we may be particularly interested in that succeeds the activity which is responsible for the psychical state of greenishness. We may say that the organism 'explodes' to green; we perhaps know of nothing more sudden than this explosive response. If we wish to demonstrate the existence of a different reaction time for the green-response, we may choose another stimulus—such as red; after the organism is stimulated for some time by red, it responds as if it were stimulated at this late period by green rays. This occurs while the eyes are open, and in spite of the fact that the stimulus red is all the time present. We may assume that, as is the case with human beings, the red grows less and less saturated and finally gives way to green. If the cockatoo is inclosed in darkness for a very short time after it has been stimulated for a few seconds by red, it responds in this case also as if to a green stimulus. Moreover, if it happens to close its eyes after it has been stimulated for a few seconds by red, it responds as if to green.

The cockatoo was trained to beat a particular group of movements, a unitary group[1] containing five elements, which was beaten in a particular tempo, (0.40 sec.) amplitude, and direction of movement when suddenly stimulated by flashing a searchlight through green glass. I shall call this a 5-group (5-g). The bird did not beat the 5-g when the light was flashed on it through a red glass; it was not trained to beat

[1]For information concerning the "unitary group of movements," see my experiments "Ueber einfache Bewegungsinstinkte und deren künstliche Beeinflussung," Zeitschrift für Sinnesphysiologie, Bd. 40, 1915

the 5-g as a conclusion to the immediate response produced by the red flash. Nevertheless, when it was illuminated for about ten seconds through the red glass and then the light was extinguished, making the room considerably darker thereby, the 5-g was regularly beaten. It evidently beat the 5-g after the green appeared as an after-response to the stimulus red.

It was accidentally observed that when the cockatoo was placed near an electric light and left unmolested until it became very quiet and closed the eyes for a period of many seconds, it almost invariably beat the 5-g very shortly after the eyes were opened. This response occurred whether the light was removed or not. I assumed that as long as the eyelids were closed, the cockatoo responded to the blood red of the illuminated blood-saturated eyelid, and that when the bird opened the eyes—thereby removing the stimulus red—it responded as if to green, i.e., the green occurred as an indirect response to the stimulus red. The responses occurred according to the following serial order: red-response, green-response, 5-g. The reaction time of the 5-g was determined by the time required for certain red- and green-responses to occur. This time was considerably shorter when only the stimulus green was used. This is in truth, such a remarkable series of responses that we must concern ourselves further with it. In the first place *we must ask ourselves what the essential differences between the red-, the green-, and the* 5-*responses are.* We will often find it valuable to use the term *group* instead of *response.* Our series would then read, red-group, green-group, 5-group. We must furthermore ask ourselves the question, *why it is that green-groups follow red-groups, and vice versa.* We know that the bird was trained to conclude the green-response with the 5-response or 5-group. *Is it not then logical to assume that red-groups are concluded soon by green-groups and vice versa because the animal has been trained to respond thus?*

Before going further, we may profitably discuss the training method which can serve to bring about such an alternation of responses as that of the red- and green-groups, and demonstrate its efficiency in any and every field of behavior. While this will be essentially a discussion of a training method, we will find ourselves continually concerned with the problem of reaction time.

The particular training method we shall now discuss is applicable to every field of activity, however simple or complex the responses in question may be, and regardless of the complexity of the subject to be trained. A few illustrations with

which every one of us is acquainted are as follows: a person smokes a cigarette and thus stimulates himself with the unusual stimulus tobacco smoke. The complicated response thus produced I shall call simply the tobacco-response. The person does not continue smoking the first time, but as to why he does not smoke one cigarette after another in rapid succession, I will not take time to discuss here. He makes a pause for the tobacco-activity, and during this pause other activities occur and become associated with the tobacco-response so that later, when the tobacco-response occurs, the second activity follows it. Whenever any two activities occur simultaneously or in a juxtaposed order, they thereafter occur more frequently in the same order. It is necessary that the adequate stimulus for only one of these activities act upon the organism, in order for both of them to occur. *The activity which is produced directly by the stimulus is an instinct; the indirectly produced one is a habit. The reaction times of instincts approach the zero value. An instinct is a response which follows immediately upon the presentation of its most adequate stimulus—and no other activity intervenes. A habit is a response which has at least one reaction time that is determined by the number and tempo of the elements of the one or more activities which intervene between the presentation of the stimulus used and the occurrence of the habitual response in which we are interested.*

When the first cigarette is smoked, at least two habits become established. The activity which by mere chance concludes the tobacco-response occurs hereafter more frequently than originally because it is now produced, not only as an instinctive response, that is, not only directly by its most adequate stimulus, but also indirectly by the stimulus tobacco-smoke. Likewise, the tobacco-response is now, not only an instinct, it is produced not only directly by the stimulus tobacco smoke, but also indirectly by the adequate stimulus for that activity—whatever this may happen to be—which terminates just as the tobacco-response begins. Later, whenever this stimulus occurs it produces directly its instinctive response which is concluded with the tobacco-response even though no tobacco smoke is present. This means that the person later responds more frequently than originally as if to tobacco smoke. He responds habitually to tobacco smoke.

If the person makes no pause for the tobacco-response the first time he smokes, this response will be in a high degree forgotten. The organic structures which the tobacco smoke excites to function can not function indefinitely. A pause inevitably comes, and if the stimulus remains always present,

not only while the activity is being carried out, but also during the recuperation pause for the structures in question, the stimulus becomes ineffective. I shall assume that it becomes ineffective because the structures which it ordinarily affects become highly exhausted, even though only temporarily. If a pause is produced artificially by removing the stimulus before the structures become in any great degree exhausted, the recuperation pause for the structures need not be very long. One essential condition that a person must fulfill in order to acquire the tobacco habit, is that he makes an adequate recuperation pause (for the structures the tobacco excites to action) before these structures become in a high degree exhausted. Another condition is that he applies the stimulus tobacco smoke each time he responds as if to this stimulus in its absence, otherwise there will occur that phenomenon of forgetting in time—or in other words, he will in the course of time become more active in other ways.

The same is true with the activity produced by fire which I shall call the fire-activity, or the fire-response. The person is active in a given way, at the conclusion of which he burns his finger with a flame. The complicated fire-response occurs and a pause is brought about for it by plunging the finger in water. Afterwards the person fears the fire, or in other words responds as if to fire when only the adequate stimulus for the preceding activity is presented. It is no longer necessary for him to place his finger in the flame in order to fear the fire; the fire-response is brought about, not only when its most adequate stimulus acts, but also sooner or later, whenever the adequate stimulus for an entirely different activity is presented to the body.

If the proper adequate recuperation pause is each time brought about before the responding structures become in any very high degree exhausted, the finger can be so trained that the physiological effects of the responses brought about either more or less directly by a slightly warm stimulus or indirectly by any other stimulus, may be so destructive that the finger will fall away. If, however, the finger is held close to the fire after it is burnt, the fire-response becomes forgotten as the structures which respond gradually become exhausted, and accordingly dissociated.

If the foot becomes very cold, and is gradually warmed, no harm results; while if it is brought suddenly near the fire, the fire-structures which have had their unusually long recuperation pause, now function so very strongly that the physiological effects may be almost the same as if the foot

were really placed in the fire. When the foot is for a long time subjected to the cold stimulus, the cold-response becomes forgotten while the structures for the fire-response are having their long recuperation pause. If the proper training method is applied to the foot, it can be caused to respond alternately as if to fire and ice, either when the adequate stimulus for one of these responses or when that for any other one that is associated with the fire- or ice-response, is presented. The essential condition which must be fulfilled in order to obtain such an alteration of responses is, the adequate pause for one must be filled in with the other response. This is possible only when each of the two responses is at least as long as the adequate recuperation pause for the other.

Ordinarily an indirectly produced response or habit is weaker than an instinct or directly produced response; but if the organism is carefully trained and care is always taken to make the recuperation pause sufficiently long, the habit may be just as strongly expressed as if it were brought about by its most adequate stimulus. For an example we choose a certain unprotected area on the body's surface at which two somewhat blunt compass points of wood can be detected as two when these stimulate points on the skin separated by a distance of two centimeters. A 5 square centimeter tactual stimulus consisting of the same material as the compass points is pressed over this area and allowed to remain 5 seconds, then a pause of 30 seconds is made before the stimulus is again applied, and so on.

Often, after only a few days' training, it is observed that the compass points now, in order to be detected as two, must be farther apart than two centimeters. As long as both stimulate points within the trained area, they are frequently perceived as one indefinite surface. This can mean nothing more than that the 5 square centimeter tactual stimulus brings about simultaneously very similar tactual responses over the entire 5 square centimeter skin area, and that these become so strongly associated in this particular order, i. e., in this temporally superimposed order, that later when a tactual stimulus produces directly one or only a very small number of these responses, such a great number of others are produced indirectly on neighboring areas that a second compass point is, as a stimulus, quite superfluous. The previously established cutaneous pattern is revived by the compass point, which is a comparatively insignificant stimulus. Careful observation teaches that the indirect responses do not occur as suddenly as the directly produced ones; but this time interval

is so very short that it is almost negligible. We can make the statement that, at least in many cases after the direct responses are produced, many of the indirectly produced responses occur so readily that a second stimulus point of the same nature as the first, when this is applied to a neighboring trained area, is very soon a superfluous stimulus. Any stimulus is superfluous if it is applied to the body after the response which it ordinarily produces directly, is, as in this case, already being carried out. If the 5 square centimeter object continually stimulates the area, no habits are established, and on the contrary this becomes an ineffective stimulus; its responses become in a high degree forgotten. This is the case with those bodily areas which come continually in contact with the clothing. Mouth areas which stimulate the tongue have become ineffective as tactual stimuli for the same reason. The eyeball which stimulates the inner area of the eyelid has for the same reason become ineffective as a tactual stimulus. These areas respond normally to other tactual stimuli which are not identical with those which have become ineffective.

If we turn now to the retina, or in other words, to a bodily surface area which is for some reason or another invaginated, and too so well barricaded against foreign stimuli that it responds primarily to certain ether vibrations, these being the stimuli which normally penetrate the schlera, lens, and humors of the eye before reaching the sensitive area in question, we find that the same laws we have been discussing are also applicable in this case. The entire retinal area has often responded to produce the sensation of reddishness, for example, when the eye is closed and turned up to the sky, or perhaps to the sun, and now when the ether vibrations which bring these responses about directly, act on a limited portion of the retina, qualitatively very similar, habitual responses are produced on the neighboring areas; those portions of the retina which are not directly stimulated respond as if directly to the stimulus red.

When the eyelid, or in other words the stimulus red is removed, the retina is directly stimulated, often by one of the many yellows or blues of nature, but it is most often stimulated directly by any one of the great variety of greens of our vegetation. The green which we may at any time see may be any of those which range from greenish yellow to greenish blue. The red- and green-responses must then necessarily become associated in this juxtaposed order, and consequently when one is directly produced the other follows it as an indi-

rectly produced response or habit. The one activitity ordinarily fills in the adequate recuperation pause for the other; thus an alteration of habit-responses is made possible.

The kind of red which we see with the eyelid closed depends upon the nature and the intensity of the light rays which strike the lid; if it is a very intense sunlight we see a reddish yellow, and under conditions of weak illumination we are often able to see a reddish blue. We should then be able to answer the question why it is that the final and most distinctive habitual response which follows any instinctive one produces the most dissimilar psychical state that we, as visual organisms, can experience. For example, the final indirect response to our common saturated red is our saturated green, and the final indirect response to this green is our saturated red.

When a person with closed eyes looks at the sun, and with the eyes still closed turns the head to a dark corner, or perhaps walks into a shady place, he sees a great variety of reds one after another. These reds become associated in this particular order. Moreover, when the observer, with the eyes closed, turns the face to the sun, or walks from a dark place into the intense sunlight, he may see every possible red—from a reddish blue to a reddish yellow. Associations are also formed in this direction. Consequently when the stimulus is our saturated red, this red is the directly produced response, and various reds as habit-responses then occur in the order determined by the length of their reaction times; those with the shortest reaction times occur first, and those with the longest reaction times occur last. This happens to be in the particular order of the similarity of the indirectly produced reds to the directly produced one; the most similar ones are produced first. For example, orange-red and violet-red have the same reaction times, with reference to the stimulus used, and are accordingly produced simultaneously and sooner than violet or orange which have longer reaction times. Since we have frequently seen various blues and yellows, as well as various reds and greens, no significant break occurs in our production of habitual responses; blue and yellow are next produced, then those with still longer reaction times, such as peacock and olive, and finally green.

During the greater part of this time the red-structures have been having their recuperation pauses, so if now the retina is stimulated in any new way, the red responses can be brought about most easily which means in greater numbers. Adequate stimuli for the green-responses are superfluous, because the structures are already responding as if to these stimuli. Fur-

thermore, the stimuli which are ordinarily adequate ones for such as the peacock- and olive-responses are ineffective, because their structures are highly exhausted. If, simply because the visual structures cannot respond in any other way, the red-responses chance to follow the green ones, these become associated in this juxtaposed order. The same thing need only occur when we use any particular green as a stimulus instead of red in order to get an alternation primarily of the red- and green-responses, in which case many of the earlier temporally intervening responses become isolated from or eliminated from the 'red-green-series' in question. We may say that they become dominated over by the red- and green-responses.

A sufficient number of these intermediate responses will always occur, however, to cause the two alternating responses to always be those which are accompanied by the most dissimilar visual psychical states.[2]

If we now turn to the unitary group of movements, we find that these behave in the same way. When two groups occur simultaneously or in close succession, the tendency is manifest for them to occur hereafter frequently in this superimposed or juxtaposed order when the adequate stimulus for only one of these is presented. Temporally superimposed habit-groups have as a rule a very short reaction time. It is for this reason that we use the term temporal superposition instead of simultaneity of occurrence. The directly produced and indirectly produced responses do not start simultaneously, but at least a part of both occur simultaneously. A few elements of the first activity may be beaten before the indirectly produced response begins.

The reaction time for the activity of a cockatoo's crying will now be our immediate subject for discussion. This is an activity, i.e. a group of movements, the adequate stimulus for which is unknown. I will therefore call it x. When the fingers were snapped in a particular way, it was noticed that the bird quickly nodded the head and then immediately afterwards uttered a shrill cry. The reaction time was in this case about 0.60 sec., and it was determined by the temporal duration of the head movement which intervened between the presentation of the stimulus and the cry. The finger snap was not the most adequate stimulus for the cry, but for the head

[2] What is here stated concerning color vision, while it may give some general hints as to the nature of the theory of vision which I shall discuss in a later manuscript, should be looked upon merely as a discussion of the problem of reaction time.

movement which had, at some time or another, become associated with the activity of crying.

The cockatoo, without having been trained to do so, beat occasionally among other unitary groups, a 7-g in the tempo of 0.77 sec. Each element of this group was a sudden stroke of the beak in the air. I choose to make this group a habit. I accordingly merely observed the bird until this particular group was beaten, and then I placed the animal on the floor and allowed it to walk about for one minute. Then I placed it on its perch (the place where it usually beat movements in this tempo) and snapped a stop-watch. The bird responded to this stimulus by suddenly jerking the body. This usually resulted in the bringing of the body, suddenly, into a more erect position. Whenever the bird did not happen to be stimulated in such a way that it beat the desired 7-g just after the body was so jerked, I snapped the watch repeatedly at intervals of five seconds until the 7-g did chance to appear very soon afterwards. When this occurred, a pause of one minute was again made for the 7-g. After this was done a few days the bird regularly beat the 7-g a very short time after the watch was snapped. The only observable activity that intervened was the jerk of the body. The 7-g was thus made a habit. It was now produced not only by its most adequate stimulus, which is unknown and which I will therefore call y, but also indirectly by the watch snap.

After the 7-g had become a well established habit, I snapped the watch, the body was straightened, the 7-g followed, and as soon as this response was finished I snapped my fingers, and then occurred a quick nod of the head, and following it the cry. This chain of activities became so well associated in the order of jerk of the body, 7-g, nod of the head, cry, that when I snapped the watch after an adequate recuperation pause had each time intervened, the responses almost invariably occurred. The reaction time of the cry was in this case, as determined by the stop-watch, 5.36 seconds. Roughly estimated, this time was divided among the various groups of the chain as follows: 0.20 sec. for the straightening of the body, 4.62 sec. for the 7-g, and on an average 0.81 sec. for the nodding of the head, and the very short time which intervened between this movement and the last element of the 7-g.

The activity of crying was a habit-group to begin with; for it was already invariably preceded by the nod of the head. It became merely a more strongly established habit when it became associated also with the 7-g. It finally became produced not only directly by its most adequate stimulus x, and indi-

rectly by the stimulus finger snap, but also indirectly by the y and the watch snap. The activity of crying accordingly occurred more frequently than previously. The training method was further applied to associate a great variety of groups with the 7-g which was regularly followed by the nod of the head and the cry. The reaction time of the cry could with a high degree of certainty be predicted if the stimulus only was known. It was merely necessary to consider the number and tempos of the movements of the various groups which regularly intervened between the presentation of the stimulus used and the occurrence of the cry.

Generally, when any unitary group is beaten, and an adequate recuperation pause for it is made, during which the animal is not allowed to beat any group in the same tempo, this group is the first one to be beaten after the pause. Since this is the case, the likelihood that the direct response to the stopwatch which snaps every five seconds will chance to be concluded just as the activity of our primary interest begins, is very much increased. The watch must not be snapped incessantly. If it is so snapped, it will become ineffective for bringing about even its most immediate response. The observed immediate response to this stimulus was very short (about 0.20 sec.) and the adequate recuperation pause was also very short, the pause being not longer than three seconds. It was therefore possible to snap the watch every five seconds without exhausting the structures which were responsible for the produced visible activity. If this direct response to the snap of the watch happens to be concluded just as the 7-g—or just as some activity which is associated with the 7-g—starts, the association is, as we have seen, more or less definitely established.

Since the most complex activities, such as the fire-, ice-, tobacco smoke-, green-, red-responses, etc. behave in just the same general way as the simple unitary groups, I have ventured to postulate the group-movement as the behavior correlate of every psychical state.

The fire-response I have spoken of as complicated. This merely means that a great variety of unitary groups are being beaten simultaneously, and serially. The stimulus fire produced directly a great number of groups, a large number of which are qualitatively similar, even though they may be quantitatively very different. They are qualitatively the same since they are beaten in a constant tempo, direction, and relatively constant amplitude of movement, and they are quantitatively the same or different in the sense that they contain

the same or different numbers of elements. For example, one of these postulated groups may contain two elements in the tempo of a hundredth of a second, and another which is beaten in the same tempo may contain fifty elements. The duration of the latter group is twenty-five times that of the former. Let us say that the 2-g is associated with a particular activity which may be called the a-response, and that the fifty-group is followed regularly by another response which may be called the b-response. The reaction time of the b-response would accordingly be twenty-five times that of the a-response; and if these groups start simultaneously, they cannot terminate at the same time.

I have ventured to speak of the behavior correlates for colors, such as red, in terms of the unitary group. I have called these the red-groups. These are qualitatively similar, but they may be widely different quantitatively. If we then fixate a piece of red paper, the stimulated visual substances beat the red-groups in great numbers. These are temporally superimposed. The shortest ones terminated must have their adequate recuperation pauses before they can be beaten a second time. And they may occur a second time before the longest red-groups are completed, i. e. provided the stimulus remains present. With any red-group there may be associated any other response. Of the possible visual responses which may be associated with the different red-responses, may be mentioned the blue-, yellow-, orange-, violet-, peacock-, olive-, green-groups, and so on. The qualitatively most dissimilar groups, i. e. the green-groups, may be associated with the longest, or under conditions, the shortest, or medium red-groups. In other words, the reaction time of the green-groups as habits may be anything the visual organism may be trained to have. The eye appendages such as the eyelid, lashes, etc., serve to make this reaction time short. This and many other things which a theory of visual sensations should explain, will be brought out in a later manuscript on "Visual Instincts and Habits."

AN ANALYTIC STUDY OF VISUAL PERCEPTIONS*

By Anna Sophie Rogers

TABLE OF CONTENTS

Chapter I

INTRODUCTION

The study of perception has had a long and involved history,—a history which extends, as we may suppose, far beyond the years of written record. The obvious dependence of life upon the use of the senses must have suggested one of the very earliest of all the human problems concerned with mind; and in due time, as we know, primitive curiosity and conjecture came to be supplemented by centuries of reflective and scientific inquiry into man's perceptual means for knowing the world. The implication of mind, however, in the direct apprehension of present objects has not by any means limited the study of perception to the psychologist. The philosopher, the logician, the anatomist, the physiologist, and the general student of life, as well as the psychologist, have derived problems from, and erected theories upon, the facts of perception. These facts have had their bearing upon the character, the trustworthiness, and the limitations of knowledge, upon the substance of reality and the nature of truth, upon the construction and the operations of the senses, and upon the mutual relations of the organism and its surroundings.

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Our own study is to be limited to the psychological aspects of perception. All of the other aspects we shall leave out of account. And within psychology itself the field of perception is so broad and the problems so diverse that we shall be obliged to propose still further restrictions upon the range of our inquiry. It is principally of the *analysis* of certain perceptions that we shall treat. We have chosen this part of the whole problem both because analysis has been neglected, even in experimental treatises, and because a description in analytic terms seems to us to be fundamental to any scientific treatment of mind.

In the following investigation we shall first approach our subject from the historical side to distinguish the old from the new and to set our own problems and methods in a clear and significant relation to others.

A. The Meaning of the Term 'Perception'

1. *Historical summary.* Until recent times two widely divergent views of perception were current in the history of psychology; two views which show a general correspondence to the ancient dualism of activity and passivity. As applied to mental facts at large, the one side of the dualism conceives of mind as an agent or power, actively manifesting itself in the world; the other side conceives of it either as a product or as a reflection of nature, but without dynamic or creative attributes. As regards perception, the one view presents a faculty by which the mind or the soul puts forth its power in the apprehension of surrounding objects; the other view presents a kind of knowledge, mediated by the senses and constructed according to a principle of association or to some law of bodily functions. The two views are to be traced through the long history of reflective thought, the emphasis placed now upon the one side and now upon the other, according to the temper of the philosophical, theological, and scientific theories of the time.

The first way of treating the problems of perception has been common since the time of Plato; the second attained its greatest prominence in the eighteenth century through the development of the doctrine of "sensationalism."

Plato[1] and Aristotle[2] are among the earliest of western philosophers to consider perception as a faculty whence the mind or soul derives its knowledge of the physical world. The inlet of this knowledge

[1] Plato, *Theatetus* (Ed. I. Burnet, Oxford), Sec. 151-152; *Republic* (Ed. Stallbaum). V. xxi; VII, vii.

[2] Aristotle, *De anima* (Ed. F. A. Trendelenburg), II, iii-xii; III, i-ii.

was by way of the sense organs, through which impressions passed. These impressions, once received, were organized by the soul (Plato) or the common sense (Aristotle).

The Platonic and Aristotelian view of perception was destined to exert an influence for many centuries. Restatements of the doctrine are to be found in Descartes, in Locke, and in Leibniz. In the system of Descartes,[3] perception is called a "function," but "function" is only the older "faculty" in disguise. And as with Plato, so with Descartes, the soul is the unifying agent for the various impressions of sense. Leibniz[4] likewise adheres to an interpretation of the soul in terms of faculties. The entire universe is composed of dynamic monads, which are ordered from the lowest to the highest according to their ability to perceive. In the human being, the monad or soul becomes a means of unification for all activities,—a conception which is strongly reminiscent of Plato and Aristotle. Perception, which ranges from obscurity to clarity, is regarded as one of the innate activities of the monad. Like Descartes and Leibniz, Locke[5] regards perception as an activity or power. He maintains that it is the primary faculty for receiving knowledge about the physical world. Not every impression upon the sense organ, however, gives rise to a perception. Whether or not it does, depends upon the frequency of its occurrence, and also upon the mind's preoccupation with other ideas. For Locke, however, perception is more than a faculty or power; it is also the material of knowledge. Through the medium of sensations the organism perceives objects as possessing the primary qualities of extension, motion, number, and figure, as well as the secondary qualities of sound, color, and the like.

By all these writers, then, perception has been regarded as a capacity, power, or faculty of mind, with the qualification that in Locke, although defined as a faculty, it is also regarded as a kind of knowledge originating in "sensation." After Locke, the notion that perception results from the combination of sense-impressions received elaborate exposition at the hands of the "associationists." Hume expressed this view when he said concerning perceptions of space and of time that "there is another very decisive argument, which establishes the present doctrine concerning our ideas of space and time, and is founded only on that simple principle, *that our ideas of them are compounded of parts which are indivisible*."[6]

Much later, Bain[7] enlarges the meaning of perception beyond that of the sensationalists. He departs from the interpretation of the latter both as to composition and as to function. Besides bare sensory impressions, muscular feelings are also included in the materials of perception, and the process becomes more highly intellectualized than before. The criterion which now determines whether a process is intellectual or sensory is discrimination, or the feeling of a difference. The fundamental fact of perception—namely, that it is the result of association,—signifies that several constituents are present and

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[3]Descartes, R., *The passions* (Ed. C. Adam and P. Tannery), Sec. xvii, xix-xxxv.

[4]Leibniz, W. G., *Monadology*, 1714, Sec. 14, 19, 21.

[5]Locke, J., *Essay concerning human understanding,* London, 1823, Bk. I; Bk. II, chap. 9.

[6]Hume, D., *A treatise of human nature,* 1748, II, iii.

[7]Bain, A., *The senses and the intellect,* N. Y., 1874, 364; *The emotions and the will,* N. Y., 1876, 549.

are accompanied by a feeling of difference between the successive or coexisting impressions. Bain anticipates James in this distinction between perception and sensation, making the difference one of kind as well as of degree of knowledge. James maintains that perceptions and sensations resemble each other, but are not identical. Perception differs from sensation in that it implies "consciousness of farther facts associated with the object of sensation."[8]

The principle of associationism was adopted by the French psychologists, Condillac and Bonnet. Condillac worked out an elaborate system of sensationalism. Every capacity of the soul is derived from sensations, between which are established associative connections.[9] He anticipates Herbart's mechanics of ideas in assuming that some impressions and presentations are favored by desire and attention, while others are kept in the background. Like Condillac, Bonnet derived all mental life from sense impressions, but instead of resulting from the association of sensations, everything was to be interpreted in terms of nervous mechanisms. The description of mind becomes a "psychology of the nerve-fibres."[10] Perception originates from simultaneous and successive excitations which give rise to sympathetic movements in connected or contiguous tracts.

In the German psychology of the eighteenth century, accounts of perception were given both in terms of faculty, as expounded by the philosopher Christian Wolff, and in terms of the sensationalism of England and France. Later, in Herbart,[11] perception—like all other mental phenomena—was reduced to the single, primary act of presentation (*Vorstellen*). It was the result of fusions and complications. Herbart's emphasis upon the fluid and the dynamic aspects of mind has exerted a strong influence in the subsequent scientific period. But it is principally Wundt's insistence upon mental analysis in terms of process, that has given us the doctrine of perception as a typical mental formation whose members are the simple processes of sensation and feeling.

2. *Recent and contemporary uses of 'perception.'* As in earlier times so also during the scientific period, the word 'perception' has been put to two unlike uses,—uses which are similar to, but not identical with, the older dualism. First, it is treated as one of the major *functions* of the mind after the pattern of the active powers or 'faculties,' and, secondly, it is more passively regarded as a *composite* of individual processes. Functionally treated perception becomes a performance, an accomplishment, or the discharge of a service;[12]

[8] James, W., *The principles of psychology,* N. Y., 1890, II, 77.

[9] Dessoir, M., *Abriss einer Geschichte der Psychologie,*. Heidelberg, 1911, 114.

[10] Klemm, O., *Geschichte der Psychologie,* Leipzig, 1911, 98.

[11] Herbart, J. F., *Lehrbuch zur Psychologie,* 1834, Sec. 55-56.

[12] These interpretations agree in general with the definitions of function as proposed by Ruckmich, C. A., (The uses of the term function in English textbooks of psychology, *Am. J. of Psychol.,* 1913, xxiv, 99); and by Dallenbach, K. M., (The history and derivation of the word 'function' as a systematic term in psychology, *ibid.,* 1915, xxvi, 473).

more passively and descriptively regarded it is an organic whole, analyzable into component parts. On the side of function the perceptual performances may be purely mental, as knowledge of environment, cognition of the world,—or it may be a psychophysical function which the whole organism expresses in overt or inhibited action. The first is the cognitive, the second the biological or psychophysical function of perception.

Among the writers interpreting perception as a composite of elementary processes,—to discuss first the analytical use—are to be found Wundt, Titchener, Angell, James, Binet. Perception thus considered is organized, *i.e.,* its constituents are arranged in definite configurations and patterns. The exact nature of these constituents varies from author to author, but there is a general agreement as regards sensational and imaginal processes, both of which vary from individual to individual and from perception to perception. Not only do images arise with sensations to form perceptions, but affections are, according to Wundt,[13] also regarded as constant, component parts. This affective component is less regarded in perception by Titchener, Angell, and James. A "pure" perception would be composed of sensations alone, but this seldom exists. There are usually imaginal accompaniments, the kind depending both upon individual constitution and upon the previous setting of similar experiences.

Again, there are other writers who propose another kind of constituent for the perceptual complex, a formal or 'funded' quality.[14] Over and above the sensational and imaginal processes, these writers profess to find something which determines the perceptual complex and which is 'subjectively' added. The chief exponents of this theory have belonged to the Austrian school, among whom are Ehrenfels, Meinong, Cornelius, and Witasek. The real value of their contention lies, not in the discovery of new and unique members or factors in the perceptual complex, but upon the fact—overlooked by traditional sensationalism—that the perception has characteristics, properties, and functions which distinguish it and which disappear when the complex undergoes analysis.

Where perception is thus regarded process-wise as a group of integrated and organized members, it is obvious that its cognitive aspect,—its meaning,—has separately to be considered. The relation of process to meaning in perception has been variously conceived. Some writers agree in regarding images as the most important factor for giving purport to the complex. According to the Wundtian doctrine of 'context,' as expounded by Titchener,[15] it is the background (either imaginal or sensational) which gives significance to the perception.

When regarded functionally, perception wears a different aspect and it provokes a different kind of scientific inquiry. Let us consider it first as a mental function and afterwards as a psychophysical func-

[13]Wundt, W., *Grundzüge der physiologischen Psychologie,* Leipzig, 1911, III, (6th ed.), 296.

[14]See Bentley, M., The psychology of mental arrangement, *Am. J. of Psychol.,* 1902, xiii, 269.

[15]Titchener, E. B., *A textbook of psychology,* N. Y., 1915, 367. *Cf.* Angell, J. R., *Psychology,* N. Y., 1908, 156.

tion. The first kind finds an exponent in Stout, who describes perception as "essentially cognition,"[16] which signifies that the mind acquires knowledge of objects by means of a cognitive performance. Stout has been influenced by James; but James was interested in the biological or psychophysical as well as in the cognitive functions of perception,[17] according to which the response of the organism or at least the motor discharge forms a part of the perception as essential as are the afferent impulses. Such a view is sustained, *e. g.,* by Dewey, who, in opposition to Stout, defines perception as primarily a fact of action, not one of cognition.[18] The perceived objects are arranged about our bodies as centers and our field of perception increases and varies with the growth and needs of the organism. From action to adjustment is a short step, and it has become common for writers whose main interest lies on the biological side to affirm that the adaptation of the organism to its surroundings is the primary function of perception.[19]

This emphasis laid upon the functional aspect of perception recurs likewise in studies in comparative and genetic psychology and in behaviorism. Watson, who stands for behaviorism as a substitute for psychology, speaks constantly of the "sense functions," which really include perception, as shown by some of the problems suggested under such headings, *e. g.,* "the rôle of vision in daily life," and "the response to ordinary sounds in the animal's environment."[20] Baldwin,[21] too, in his statement of genetic problems, emphasizes the fact of "functional epochs," or the child's acquisition of knowledge through direct experience.

We may, then, summarize our historical sketch by remarking upon the persistence through many centuries of the 'active' and 'passive' views of perception. Until recent and more scientific times perception was either a power, a faculty, of the soul, or else the mere reception of 'impressions' to be connected and elaborated by 'association' or by the nervous system. Under the conjoint influence of the other sciences and of experiment within psychology itself, the dualism of active and passive has been modified, but it still persists in the functional and analytical accounts of perception. The one looks upon perception as a mental or psychophysical performance, of use either to knowledge or to the organism; the other describes it in terms of integrated processes.

With these gross distinctions, historical and current, in view, we turn to an inspection of the actual problems and methods which are to be found in the recent literature of our

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[16] Stout, G. F., *A manual of psychology,* N. Y., 1899, 241.

[17] James, W., *The principles of psychology,* 1890, II, chap. xix.

[18] Dewey, J., Perception and organic action, *J. of Philos., Psychol., Etc.,* 1912, ix, 645.

[19] Judd, C. H., What is perception? *J. of Philos., Psychol., Etc.,* 1909, vi, 36; Aaronson, I., Perception, *ibid.,* 1914, xi, 37.

[20] Watson, J. B., *Behavior,* N. Y., 1914, 33-36.

[21] Baldwin, J. M., *Mental development, etc.,* N. Y., 1895, 1ff.

subject. But in order to describe these problems and methods we must first glance at the classifications of perception to be found among modern psychologists. Our inspection of kinds, problems, and methods, should be a useful means to orientation within the field of our own experimental studies which we are presently to enter.

B. Perception in Modern Psychology.

1. *Kinds and classes.* In order to discover an adequate basis of classification, we may review the treatment accorded our subject by such systematic psychologists as Wundt, Külpe, Titchener, Angell, Pillsbury, Stout, and Ebbinghaus. Consonant with our historical summary, we find, in these systems, that perceptions are classified either (a) as modes of integration of component processes or (b) according to the kinds of knowledge which they mediate.

(*a*) *Classification of perceptions as modes of integration.* Wundt is one of the chief exponents of a distinction made in terms of the manner of combination of the elementary processes. Perceptions are, for him, fusions, complications, and assimilations.[22] An association of elements by fusion may be intensive or extensive. By intensive fusion he means that interconnection which exists between processes by virtue of their qualitative relationship, *e. g.,* the clang. This kind of affinity between mental processes exists only among those arising from the same sense-department and from sensory stimulation. The term 'fusion' is extended to other forms, spatial and temporal, where it is designated as extensive. If the perception, on the other hand, arises from a union of sensations from different sense-departments, as a perception of water from visual and cutaneous processes, then it is known as a complication. These two modes of integration account for perceptions that are composed of only present experiences. There are, however, other perceptual complexes in which reproduced experiences play an important part, as the perception of any familiar object which contains more than the bare sensory processes. Whenever image combines with sensation to form a perception, the integration is known as an assimilation.

Külpe and Titchener also classify perceptions according to the mode of integration of the component factors. The modes of perceptual integration according to Külpe differ but slightly from those of Wundt. Külpe's classification includes fusion, colligation, and association.[23] Fusion is defined as a qualitative connection between elements when they are spatially and temporally identical, *e. g.,* if two notes were simultaneously given, the tones would be temporally identical but qualitatively diverse. Fusion, then, from Külpe's point of view, is only one (the intensive) phase of Wundt's fusion. The spatial and temporal perceptions are designated by Külpe as colligations. For example the tone a^1 repeated with alternately long and short intervals, would be an instance of identical quality but of different temporal

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[22] *Grundzüge,* III, (6th ed.), 500ff.
[23] Külpe, O., *Grundriss der Psychologie,* Leipzig, 1893, 284ff.

relations and it would fall accordingly into a temporal colligation, rhythm. A pattern of colors would illustrate the spatial kind of colligation. Furthermore, elements not only unite qualitatively, spatially, and temporally, but they integrate also because each tends to establish a relationship between itself and other processes. So arise associative combinations.

According to Titchener, perceptions are distinguished as qualitative, spatial, temporal, and mixed.[24] The first three would all be grouped as fusions, intensive and extensive, by Wundt, save that Titchener's qualitative complexes also include Wundt's complications, and Titchener's mixed type is virtually equivalent to Wundt's assimilation.

In general, then, with slight limitations and expansions of terminology, Wundt, Külpe, and Titchener make similar classifications of perception. Their distinctions between kinds or types depend mainly upon the capacity of the sensory processes to integrate qualitatively, spatially, or temporally, or upon the fact of images attaching themselves to sensations to form perceptions.

(b) *Classification of perceptions based upon kind of knowledge.*

A classification of perception from this point of view depends upon an interpretation of it from its functional aspect. Stout, Angell, Pillsbury, and Ebbinghaus treat it in this manner. Stout enumerates five kinds: perception of external or physical reality, of space, time, causality, and 'thinghood.'[25] A glance at these terms will disclose the emphasis placed upon the cognitive function. In order to have any perception at all there must be some external or physical object, even the body or its parts becoming external to the self when cognized. Besides being external, every object must possess unity, identity, and independence, by virtue of which it is perceived as distinct from every other object. This distinctness is designated 'thinghood.' Furthermore, an object, besides being external to the organism and distinct from every other object, may also be thought of as causality, or as the result of a gradual, practical adaptation, whereby through past experiences the organism becomes aware of the object's efficiency or inefficiency. In all of these categories of perception the importance of knowledge or information about objects is the outstanding factor. Likewise, perceptions of space are such as have to do with information about some physical object, for space is a matter of relations in position and distance, and position and distance must pertain to objects. What is true of spatial perceptions is also true of temporal, *i. e.,* time bears relation to something, as to the past, present, and future.

Angell, Pillsbury, and Ebbinghaus likewise draw distinctions between the large classes of perception according to the kind of knowledge derived. Angell, however, limits all perceptions to two main classes, spatial and temporal. Every perceived object implies a spatial and a temporal order. Each class has its own peculiar function: by means of spatial perceptions the organism becomes accommodated to a three-dimensional world; by means of temporal perceptions, to a "world of sequential events."[26] Thus, by virtue of knowledge concerning the outside world acquired through the perceptual functions the living being adjusts itself to its environment.

Pillsbury does not vary much from Angell's position. The chief concern is, he says, the tracing of "factors that aid in the transforma-

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[24] *Op. cit.,* 389.
[25] *Op. cit.,* 312.
[26] Angell, J. R., *op. cit.* N. Y., 1908, 172.

tion of the sensations into objects."[27] During this procedure, it is also necessary to keep in mind the way in which "elementary mental states come to mean that which they are not." For him, the meanings, or informations, are of much more importance than the formation or integration of the perception. The kinds of meaning, or knowledge, which are obtained by way of the perceptual process are fusions, and spatial and temporal relations. Besides these, the mind may also become aware of movement of change, and of rhythm. The former is related both to space and time, the latter primarily to time. We see, then, that these types of perception are not essentially different from those of Angell and Stout. They stand related to those of the former in that fusion, movement, and rhythm are perceived as parts of a spatial and temporal order; to those of the latter by virtue of the fact that in so far as adjustment and reaction to objects greatly aid in the perception of them, objects as perceived, whether as fusions, movement, or rhythm, must possess physical reality, causality, and 'thinghood.'

Finally, the classification of Ebbinghaus, although it appears to be of the first or integrative kind, is at bottom functional. The general (*gemeinsame*) or formal attributes, unity, identity, plurality, difference, and the like,[28] which Ebbinghaus assigns to sensations, are really functional marks, so that his account of perception is really not at all a description of mental processes but a description of the kinds of knowledge to which perceptions give rise. Ebbinghaus has therefore to be grouped with Stout, Pillsbury, and Angell, rather than with Wundt, Külpe, and Titchener.

In general, then, it may be said that those psychologists who base their classification of perception upon kind of knowledge derived are emphasizing the functional aspect. Furthermore, this division of the types of perceptions may be as various, with respect to terminology, as the relationships of object to object and of object to organism may suggest. On the other hand, those who have distinguished classes of perception upon the basis of integration have placed the main emphasis upon the fact that perception is an organized complex made up of elementary processes.

The recent insistence in some quarters upon the adaptive functions of mind leads the student of perception to look for a third kind of classification which should take into account the facts of organic adjustment. Systematic work of this kind seems, however, not to have been done. Where general terms have been demanded by the biologizing psychologist, the older, cognitive distinctions seem to have been transferred,—by a comfortable logic,—to the ecological kind of function.

2. *Problems of perception.*[29] With this historical back-

[27] Pillsbury, W. B., *The fundamentals of psychology,* N. Y., 1916, 269.

[28] Ebbinghaus, H., *Grundzüge der Psychologie,* Leipzig, 1911, I, 442.

[29] For a basis of determination of the problems and methods of perception, approximately 150 investigations have been examined. For this purpose, the studies upon perception reported in the following

ground in view, let us turn to the empirical problems which the subject of perception has presented, and then proceed to the methods employed for their solution. The problems may be grouped under the following eight headings:

i. Dependence of perception upon variations of stimulus as to its (a) kind, (b) temporal peculiarities, (c) spatial arrangement, and (d) degree; perception is either (1′) analyzed in terms of process or (2′) referred in terms of meaning to variations in stimulus.
ii. Dependence of perception upon organic conditions: (a) general, and (b) local.
iii. Dependence of perception upon the general state or condition of mind.
iv. Relation of perception to organic movement.
v. Deranged perceptions: synaesthesias, illusions, hallucinations.
vi. Relation of perception to thought.
vii. Nature of perception in animals.
viii. Development of perception: (a) phylogenetic, and (b) ontogenetic.

An examination of the recent experimental literature will serve both to define and to illustrate the numerous perceptual problems which have already been formulated. We shall take them in order.

i. Dependence of perception upon stimulus

The problems of perception which depend upon the control of the stimulus present many and several phases, since the stimulus may be widely varied with respect to its various characteristics. The task then resolves itself into (1′) the analytical description of the sensational and imaginal processes making up the perception under variations of stimulus, or (2′) the correlation of change in meaning with change in stimulus. Either of these modes of interpretation of results may be the aim of the investigation under any of the four possibilities of modification which have been distinguished.

(a) Dependence of perception upon change in kind of stimulus. Under this sort of quest the three remaining aspects of stimulus, time, arrangement, and degree, are kept as constant as possible, while modifications in kind are made under control, in order to determine any difference in the perception which may result. The investigation by Kemp[30] into tonal fusion furnishes an excellent example of this

periodicals were scrutinized: *Am. J. of Psychol.*, 1903, xiv—1916, xxvii; *J. of Animal Behav.*, 1911, i—1916, vi; *J. of Philos., Psychol., Etc.*, 1910, vii—1916, xiii; *Psychol. Rev.*, 1908, xv—1916, xxiii; *Brit. J. of Psychol.*, 1904-05, i—1915-16, viii, Pt., 3; *L'Annee psychol.*, 1910, xvi—1914, xx; *Arch. f. d. ges. Psychol.*, 1909, xiv—1915, xxxiv; *Psychol. Stud.*, 1905-06, i—1907, iii; 1911-12, vii—1913-14, ix; *Psychol. Index*, 1915, xxii (all references quoted upon perception); and many other volumes and periodicals not here quoted.

[30] Kemp, W., Methodisches und Experimentelles zur Lehre von der Tonverschmelzung, *Arch. f. d. ges. Psychol.*, 1913, xxix, 139.

part of the first problem. Two notes of unlike vibration rates were presented to the observers, who were instructed to compare the tonal complexes as regards degree of fusion. The introspective reports, however, were not highly analytical.

Experiments in which mere changes in meaning are reported, and in which the aim is to refer these changes to modifications in the character of the stimulus are illustrated by Woodrow's[31] study of rhythm. The influence of the intensive and temporal factors was minimized or eliminated, thus producing the most favorable conditions possible for correlations between qualitative changes in perception and variations in the nature of the stimulus.

(b) Dependence of perception upon the temporal properties of the stimulus. Perceptions of rhythm and intervals of time are directly influenced by any temporal modifications in the stimulus. Experiments which illustrate this were devised by Ross[32] and Wallin.[33] In the first investigation, a series of ten clicks, all at constant intervals with one exception, wherein the time was shortened, was provided. The task of the observer was to detect the exceptional interval. The perception of this particular duration was described only by its detection, when compared with another, constant time, *i. e.,* it was described only in terms of meaning, not of process.

On the other hand, Wallin determined not only changes in meaning, but also the processes which underlie the perception. The observers were asked to find a tempo midway between two others, one fast and one slow. With the quantitative results were also recorded comments made by the observers upon the aids employed in the perception and the determination of the temporal rate. Wallin reports, *e. g.,* that "four believed that they based their estimations on the immediate impressions," "practically all the subjects made use of the kinaesthetic factors," or again, "only three were conscious of using any visual imagery."[34] Although these comments are not true introspections made in terms of process, they show more of a tendency in that direction than the reports of the observers in Ross' experiment.

(c) Dependence of perception upon the spatial arrangement of stimulus. The possibilities of modifications in the spatial pattern of stimulus are illustrated in many investigations into those perceptions which owe their configurations and meanings primarily to the spatial arrangement of the exciting agency, *e. g.,* perceptions of lines, depth, size, and form. Such an experiment as Cook carried out upon the cutaneous estimation of filled and unfilled space, is a case in point. Aesthesiometers for punctual and continuous impressions were used.[35] The problem—the determination of the amount of illusion present in the comparison of filled and unfilled spaces—assumes a change of perception corresponding to a change in the spatial relationships of the stimulus.

We find, however, other investigators describing the perception both as meaning and as process. In an experiment upon reversible

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[31]Woodrow, H., Rôle of pitch in rhythm, *Psychol. Rev.,* 1911, xviii, 54.

[32]Ross, F. B., The measurement of time-sense as an element in the sense of rhythm, *Psychol. Monog.,* 1914, xvi (No. 69), 166.

[33]Wallin, J. E. W., Experimental studies of rhythm and time, *Psychol. Rev.,* 1912, xix, 271.

[34] *Ibid.,* 295.

[35] Cook, H. D., Die taktile Schätzung von ausgefüllten und leeren Strecken, *Arch. f. d. ges. Psychol.,* 1909-10, xvi, 442.

drawings, Becher,[36] *e. g.*, has discovered not only that different meanings arise under spatial differences of stimulus but also he has identified the mental processes which correspond to these differences.

(d) Dependence of perception upon the degree of stimulus. Such an investigation as was conducted by Arps and Klemm[37] upon the relation of intensity to the localizing of sound will illustrate this point. The source of the sound was a tuning fork, which could be moved in either the median or transversal plane of the head. The sound itself was transmitted to the observer by means of a telephonic connection. The observer's task was to compare one sound with another of like physical intensity but in a different position, or with a sound at the same distance as the first, but of unlike intensity. Since the duration and equality of the sound were constant, its intensity was the determining factor for localization. Since the investigators were concerned with spatial localization, their problem was of the second or cognitive type.

ii. Dependence of perception upon organic conditions: (*a*) *general, and* (*b*) *local.*

(a) The perception of an object may be greatly influenced by the general state of the organism. For example, one experimenter, Jones,[38] permitted himself to be anaesthetized with chloroform in order that he might introspectively observe the effect upon his mental processes. He found not only that perceptions were the first mental processes to drop out, but that these also changed as they disappeared. For instance, he says that "all movements made appeared to be much longer than they actually were," that "winking gave the peculiar feeling of a great curtain slowly shutting out the light and as slowly rolling back again," and numerous other instances of perceptions which were modified by the anaesthetized condition.

(b) Every perception depends upon the condition of its particular sense organ, consequently modifications of local organic conditions result in modifications of the mental correlates. Perceptions which indicate directly such an influence are the difference in the perception of size by different parts of the retina, the perception of the position of objects with the head held at various angles, and the different perceptions of the same pattern upon various parts of the skin. The first depends upon the local conditions of the retina; the second, upon organic conditions within the head; the third upon the distribution and 'local sign' of the pressure organs in various parts of the skin. For illustration, we may cite Stevens' experiment upon peripheral vision.[39] By use of one of the psychophysical methods, the author determined the perceived difference in the size of the objects at different parts of the retina.

iii. Dependence of perception upon the general state or condition of mind.

States of this sort are attention, expectation, doubt, hesitation, and

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[36]Becher, E., Ueber umkehrbare Zeichnungen, *Archiv. f. d. ges. Psychol.*, 1909-10, xvi, 397.

[37]Arps, G. F., and Klemm, O., Untersuchungen über die Lokalisation von Schallreizen, *Psychol. Stud.*, 1912-13, viii, 226.

[38]Jones, E. E., The waning of consciousness under chloroform, *Psychol. Rev.*, 1909, xvi, 48.

[39]Stevens, H. C., Peculiarities of peripheral vision, *Psychol. Rev.*, 1908, xv, 69.

deliberation. The subject is so predisposed when the stimulus is applied as to affect his perception. In reporting an experiment upon the influence of expectation upon auditory localization, Geissler says[40] "in our method the greatest importance must be laid on the instructions given to the observers and the subsequent reports demanded from them." In other words, he had previously prepared his subjects to perceive the object in a certain position and his task was to determine in how far this mental set affected the localization of the sound.

iv. Relation of perception to organic movements.

In the three groups just reviewed, the task of the investigator ended when he had established and described a perceptual complex under certain conditions of stimulus, body, or mind; but in the fourth group emphasis is placed less upon sensory and imaginal components of the perceptual consciousness and upon conditions than upon resultant and motor tendencies which are sometimes alleged to be factors essential to perception.[41] The following quotation from Judd, who writes in terms of 'reaction' and 'adjustment,' will make the matter clear. "The simplest perception of an object which is presented to the eyes contains a great deal more than sensory elements of which it is composed. It consists of certain forms of arrangement and certain tendencies toward reaction which must be recognized by any student who would work out an adequate account of these processes."[42] Partial or total 'adjustments' are then, for Judd, necessary factors in perception.

From what has just been said concerning the relation of perception to organic movement, it is evident that such writers as Judd are less concerned with the perception itself than with events and occurrences concomitant with, or subsequent to, the perceptual complex, with events which are really the final result and not the components of perception. The chief interest in perception, from this point of view, is a consideration of it as the accommodation of the organism to its surroundings.

v. Deranged perceptions; synaesthesias, illusions, hallucinations.

Some of the problems developing from the study of deranged perceptions are: (a) a description of the processes which comprise the complex; (b) a description of meanings; and (c) an explanation of the cause of such perceptions. This last problem belongs primarily under the second group of problems, or the dependence of perception upon organic condition. A description of a deranged perception in terms of process is difficult, because of the inability and the limited reliability of introspective reports under conditions where such derangements occur. But referring again to the experiment by Jones[43] upon the influence of anaesthetics, we find from his own observations that the distorted perceptions are the result of disturbed sensory processes, *e. g.,* he reported that, at first, the visual sensations were clearer and more intensive, while auditory and tactual processes decreased in clearness, and his deranged perception of movement was the probable sequence of the low intensity of tactual sensations.

[40]Geissler, L. R., Sound localization under determined expectation, *Am. J. of Psychol.,* 1915, xxvi, 269.

[41]Dewey, J., *op. cit.,* maintains that perception is the result of incipient and partial organic responses.

[42]Judd, C. H., *op. cit.,* 40.

[43]Jones, E. E., *op. cit.,* 51.

vi. Relation of perception to thought.

The relations of perception and thought are to be sought for on the side of common properties either of process or of function. With regard to composition, we might expect to find like components and even similarities of integration. Thought would then simply become a complex involving greater elaboration of meaning than perception, due to its greater complexity of composition and differences of function of the component processes. Thought would, from this point of view, be distinguished from perception by degree of meaning rather than as a different kind of mental phenomenon. On the side of function common attributes and properties of the two activities must be taken into account. If perception is considered as cognitive, and thought as elaborative, then there is a wide distinction; but if we look upon perception as accomplishing something more than mere apprehension, then we find that the two functions again approach each other in nature. The problem, however, has never received comprehensive treatment.[44]

vii. The nature of perception in animals.

Here we find two main problems; first, a description of the mind of organisms below man based upon a comparison with the human mind, and secondly, the relation of animal performances to perception. An experiment by Johnson[45] will illustrate the first, in which the ability of monkeys and chicks to discriminate lines or striae of different widths was made the subject of investigation. Limens of discrimination were found for these animals as well as for man. Certain other persons, however, who are chiefly interested in the animal's performance lay no stress upon the analysis of the perception itself. A description in terms of performance, behavior, or response which is directly correlated with physical conditions, suffices for their purposes. We find Watson [46], *e. g.,* taking this extreme view.

viii. Development of perception; (a) phylogenetic and (b) ontogenetic.

The first group of problems listed here presents a large field and a wide range. The investigator who enters upon such a task as determining the development of perception in the animal phyla must possess much ingenuity and versatility in pursuing the many problems which present themselves and in devising methods for their solution. Here would be listed such problems as the modification of perceptual complexes from the lowest to the highest living form, a comparison of differences in perception with differences of nervous system, and a comparison of perception with other mental complexes at the various stages of development in the animal series. The ontogenetic development of perception, on the other hand, does not include such a wide scope, but limits itself to the study of perception within one organism from birth to maturity. Judd and Cowling[47] present a

[44]F. Aveling (Relation of thought-process and percept, *Brit. J. of Psychol.,* 1911, iv, 213) considers the interrelations of thought and the sensational processes as the problems which underlie the relation of thought and perception.

[45]Johnson, H. M., Effective differences in width of visible striae for the monkey and the chick, *J. of Animal Behav.,* 1916, vi, 169.

[46]Watson, J. B., *op. cit.*

[47]Judd, C. H., and Cowling, D. J., Studies in perceptual development, *Psychol. Rev., Monog. Sup.,* 1907, viii, No. 34, 349.

meagre account of the gradual development of a perception in their experiment on the rate of learning to reproduce certain outlined figures. The reproduction was performed, first, with the eyes closed; secondly, with the eyes open and the drawings hidden, and thirdly, under visual control both of the movements and of the drawings. We have already noted (Sec. *iv*, above) that Judd is primarily interested in perception as described in terms of 'reaction' and 'adjustment,' and so again we mark the characteristic failure of introspective description in the experiment just mentioned.

If we glance in review, then, over the problems which are concerned with perception, we see that they come under the eight headings enumerated, which may be further divided, in turn, into two large groups. The first would include the first four problems; the second, the last four. The first would treat of perception in its general relations (a) to the environment, physical and bodily, and (b) to psychophysical preparation or predisposition. The last four groups may be classed together as treating of specific problems: (a) derangements of perception, (b) relation of perception to thought, (c) perception in animals, and (d) the development of perception in the individual and in the race.

3. *Methods of investigating perception.* We have seen that the problems of perception are varied and numerous, and that they correspond to different interests, different points of view, and different modes of systematic formulation. Now as we advance to the discussion of methods and means of solution we may reasonably expect a like variety. The means which have actually been used in the study of perception we have collected from the journals and the monographs, and we find that they may be arranged under the following six categories: (1) logical, (2) introspective, (3) psychophysical, (4) comparative, (5) genetic, and (6) behavioristic. The interpretations put upon these terms agree with those proposed by Ruckmich.[48] The first is a rational principle applied for the sake of interpretation; the second is 'method' taken in the narrower sense; the third, a mode of procedure employed under experimental methods; and the fourth, fifth, and sixth are, primarily, points of view.

i. The *logical method* is the term which I have used to include all those procedures whereby unanalyzed experiences have been brought together in the mass and by reflection interpreted. It is the sort of means which an investigator uses when he reasons out descriptions of mental experiences without subjecting them to experimental control or even to experimental identification. It is a general and uncontrolled

[48] Ruckmich, C. A., A schema of method, *Psychol. Rev.*, 1914, xxi, 401.

manner of investigation to which psychologists still resort. To illustrate the point, we may refer to Aaronson's[49] interpretation of the relation of perception to knowledge and action, wherein he contends without experimental verification that a man of the dullest type would show more knowledge than the most perfectly contrived mechanical being because he is able to comprehend the meaning of the perception and suitably to adapt himself thereto. But there is no hint of experimental control and no test of presuppositions. Again, an application of this same method is shown in Grünbaum's[50] analysis of the psycho-physiological nature of visual impressions of movement in primitive man, where analogies are drawn between structure and mental event and between modern and primitive man. Nevertheless,—in spite of its limitations,—the logical method of interpretation and investigation finds a place in solving just such problems as the last mentioned, where experimental control is impossible. It finds further application, too, in the description and interpretation of conscious moments, which are common to a great mass of people, *e. g.,* the conception of an international war which brings the whole reading world into a single group. Here again, a direct survey by experiment would hardly be feasible. But in the problems of perception the logical method should occupy a subordinate place.

ii. *Introspection* is the one method peculiar to psychology alone. By it we mean the direct, controlled observation of mental phenomena for purposes of analysis and description. From these immediate observations the experimenter makes interpretations and correlations of fact. In practice, however, the introspective method is interpreted in two distinct ways: first and primarily, to analyze and describe mental processes; and secondly, to indicate and estimate meanings and relations, *i. e.,* to give information concerning objects and to comment upon the progress of events.

(a) The first application of the method is the one which has received the sanction of modern systematic psychologists, since every kind of mental event presumably lends itself to this means of investigation. There is—as its champions maintain—no single kind of psychical material to which introspection is limited, and therefore, facts gained by any other method may be substantiated by a direct inspection of mind. Titchener has defined and outlined a schema of the introspective method[51] in which he maintains that introspection includes "an attention from the standpoint of psychology, and a record in the terms and under the captions of psychology." He further proceeds to distinguish two forms of introspection, the direct and the indirect; the first being an immediate description of the processes, the second, a description made upon the basis of reproduced processes, or the memorial image. Titchener's account of introspection substantially agrees with G. E. Müller's critical exposition of the preceding year.[52]

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[49]Aaronson, I., Perception, *J. of Philos., Psychol., Etc.,* 1914, xi, 37.

[50]Grünbaum, A., Ueber die psychophysiologische Natur des primitiven optischen Bewegungseindrucks, *Folio Neuro-biol.,* 1915, ix, 699.

[51]Titchener, E. B., The schema of introspection, *Am. J. of Psychol.,* 1912, xxiii, 491.

[52]Müller, G. E., Zur Analyse der Gedächtnisstätigkeit und des Vorstellungsverlaufes, *Zsch. f. Psychol. u. Physiol. d. Sinnesorgane,* 1911, Ergbd. v, 64. In describing the method, Müller says, "Von Selbstwahrnehmung rede ich überall da, wo in Beziehung auf einen psychischen Zustand durch unmittelbare Auffassung desselben oder durch Erinner-

(b) The term introspection, used as a description of meanings, as a designation of knowledge-about-an-object, or as running comment upon an experiment does not analyze in terms of process. By its use the observer reports fragmentary knowledge of objects and conditions. Take, for example, the problem of the relation of thought to perception investigated by Aveling, in which the observers were subjected to certain conditions which they reported as 'typical' or 'individual.' No attempt was further made to analyze the processes which carried this meaning. A few samples from the observations of Aveling's[53] observers will bring out the difference between facts gained through the informative method and the materials of introspection strictly taken. The mere description of the meaning is made evident by the following: "I did not see that as a type of a class. It was a letter-scale. The bars were yellow, the support black. I cannot draw it; for the meaning of the instruction came to me and troubled me. There was an inhibition. No word came to consciousness. I at once thought of Nardis' machine. He had one." . . . "It was a pair of bluish nippers. No word came to consciousness. They were closed. I think I saw it as a picture and as type."

The 'informative' variant of the method, which principally records running comments upon the experiment, falls as far short of describing consciousness in terms of processes as does the related type just illustrated, the one which yields bits of knowledge or reflection. No interpretation can be made concerning the actual composition of perception from confessions of the following character. "I can't say much of the first stage. The picture rolls on, but doesn't take a final interpretation at first. I feel there is a preparation for a definite final something. I had confidence of something definite which would come up when the picture was gone. Then I saw other parts, and the previous interpretation was choked down by this new sensation, and so the other never appeared in full consciousness." . . . "it's an awful effort to look at one thing. It's easier to be passive than active."[54] Such reports do not so much describe objects as merely comment upon the attitude of the observer during the period of experimentation or upon the course of the experiment itself.

iii. *The psychophysical methods*[55] have played an important rôle since Fechner's time. They are peculiarly adapted to the study of relationships between stimulus and sensation. Since perception has its origin in the direct stimulation of receptor organs, all modes of procedure which may help to illuminate the relations obtaining between

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ung an denselben etwas konstatiert wird, . . . Wenn es sich um die Schilderung eines äusseren Gegenstandes handelt, kann die Beschreibung entweder auf Grund gegenwärtiger Wahrnehmung des Gegenstandes oder auf Grund der Erinnerung an eine oder mehrere frühere Wahrnehmungen des Gegenstandes erfolgen. Findet die Beschreibung auf Grund gegenwärtiger Wahrnehmung statt, so steht es stets so, dass die zur Schilderung gelangenden Eigenschaften oder Teile des Gegenstandes sukzessiv die Aufmerksamkeit besitzen und ihren Besonderheiten in allgemeinen angemessene, von sprechenden Worten begleitete Apperzeptionen (Erkennungen) erfahren."

[53] *Op. cit.*, 221.

[54] Smith, F., An experimental investigation of perception, *Brit. J. of Psychol.*, 1913-14, vi, 327, 333.

[55] For discussion see Titchener, E. B., *Experimental Psychol.*, II, Pt. II, Introd., N. Y. 1905.

stimulus and the attributes of sensation may be given a place in the study of this topic. The psychophysical methods have most frequently been applied to perceptions in the visual, auditory, and cutaneous fields, but are widely applicable where quantitative results are sought. An illustration of a psychophysical mode of procedure would be such an experiment as the determination of the influence of accommodation and convergence upon the perceptions of depth,[56] wherein the amounts of accommodation and convergence may be accurately measured and correlated with the degree of depth perceived; or, the determination of the influence of expectation upon localization of sound by the number of errors made.[57]

All the methods so far discussed are those peculiar to the psychology of the human individual. The other three modes of procedure which have helped to solve problems of perception are the comparative, genetic, and behavioristic. All of them may be applied to organisms below man as well as to man himself.

iv. *The comparative method* is a mode of procedure peculiar to itself, namely that of inferring animal mind from human mind. One necessary step is a comparison of the animal and human forms in respect to structure, functions, and behavior. When an animal acts in a given manner under given conditions, the investigator constructs the animal mind on the basis of a comparison with human mind placed under similar circumstances, but with due allowances made for differences of structure, function, and manner of living. The literature upon animal psychology reveals many illustrations of this method. A representative instance is furnished by the article by Johnson already cited.[58] Johnson first determines the differences necessary in certain patterns to be perceived as discrete both by man and by the monkey and the chick. He then constructs the mind of the lower animals on the analogy of the human mind.

v. *The genetic method* may likewise be applied to animals. An instance of an investigation carried on to determine the phylogenetic relationship of various organisms is furnished by Hamilton.[59] By subjecting various beings, as men and children of various ages, monkeys, cats, dogs, and a horse, to the same problem under the same conditions, a comparison was attempted between the reactions of the same species at different ages, and between animals of different genera. This manner of approach is also applicable to problems dealing with the development of the individual. For example, the experiment of Judd and Cowling[60] referred to above, presents the problem of the genetic growth of perception in man with suggestions for a method for its solution.

An investigation of mind from the genetic point of view is advocated by Kirkpatrick, Yerkes, and Baldwin. According to Kirkpatrick the methods of genetic and comparative psychologists are similar. He believes that the student interested in mental growth must first know the structure and behavior of the simpler organisms and then,

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[56]Baird, J. W., The influence of accommodation and convergence, etc., *Am. J. of Psychol.*, 1903, xiv, 150.
[57]Geissler, L. R., *op. cit.*
[58]Johnson, H. M., *op. cit.*
[59]Hamilton, G. V., A study of trial and error reactions in mammals, *J. of Animal Behav.*, 1911, i, 33.
[60]Judd, C. H. and Cowling, D. J., *op. cit.*

by inference, construct its mental life.[61] Yerkes,[62] likewise, implies the use of inference in the investigation of individual and racial mental development. Emphasis, however, is placed repeatedly upon the observation of organisms, especially upon the observation of behavior and its adaptive functions.

In Baldwin,[63] we find an entire program outlined and carried through. He assumes that mental growth runs parallel to the development of the nervous system. The task involves a determination of the amount of mental development at various levels in the individual. The suggested manner of approach is that of the dynamogenic method,[64] or an interpretation of the development of the mind by its reflection in movement.

vi. *The behavioristic method* does not attempt to describe mental phenomena as does the comparative method. In its extreme form it seeks only to describe movements and to refer them to antecedent stimuli. An illustration of this method is to be found in an experiment by Bingham,[65] wherein no interpretation of mental processes is attempted. Even in the milder forms of behaviorism, perception is scarcely a problem in itself, but only in so far as it is implied in the description and explanation of organic processes. And in the most uncompromising forms of this branch of ecological study, there is no place at all for the problems of perception.[66]

Thus we find that various paths of approach are available in connection with the problems of perception; though only one of them arrives at actual description in terms of process and form of integration. Our six methods fall into two natural groups. The first group, which includes the logical, introspective, and psychophysical forms, involves in various ways the individual whose mind is under investigation. The psychologist who makes use of the logical method bases his descriptions upon a reference, more or less explicit and exact, to facts taken from his own or from similar experiences; the psychologist who employs the psychophysical methods is dependent upon the judgments of the observer; while the introspectionist finds his facts of perception by immediate observation.

The second group, which includes the comparative, genetic, and behavioristic forms, is characterized by a primary dependence upon the observation of overt actions or of other organic activities. From this common starting-point, the three

[61]Kirkpatrick, E. A., *Genetic psychology,* N. Y., 1910, 3.

[62]Yerkes, R. M., *Introduction to Psychology,* N. Y., 1911, 212.

[63]Baldwin, J. M., *Mental development in the child and the race,* N. Y., 1895.

[64]*Ibid.,* 43.

[65]Bingham, H. C., Size and form perception in Gallus domesticus, *J. of Animal Behav.,* 1913, iii, 65.

[66]Pillsbury (*op. cit.* 14), however, maintains that facts gained from the observation of behavior should be substantiated by introspection.

diverge in their several interpretations of behavior. The first emphasizes the comparison of minds of different forms, the second, the development of mind, and the third, the correlation of response with stimulus.

Chapter II

EXPERIMENTAL INVESTIGATIONS

A. An Analysis of Perceptual Complexes

Problem and method. The main purpose of these investigations has been to obtain a descriptive analysis of perception. In the first series of experiments the primary object was to study certain fairly simple perceptual formations, laying special emphasis in our description upon (1) the kinds of process involved, upon (2) the modes of their integration, and upon (3) the temporal sequence of the component members. For this purpose, twelve series composed of ten irregular inkblots[67] were used. There were five observers, C, Ra, Ru, S, and V.[68] The observer (O) was seated four feet from the exposure apparatus, a modified Whipple tachistoscope, which was employed throughout the experiments. Two seconds after a 'ready' signal, a card, bearing an irregular inkblot (about 3 cm. x 3 cm.) was exposed for .2 sec. The following instructions were given to O.

> "Two seconds after a preparatory signal is given, a visual field will be exposed. After the exposure, an auditory distraction-stimulus will be sounded. At the signal, fixate and attend to the cross. After the exposure period, give (1) an introspective account of all mental processes occurring between the beginning of exposure and the distraction-stimulus, and (2) a verbal description of all objects perceived."

At each sitting, a series of ten cards was completed. The twelve series were repeated twice, wholly or partially, for each observer, once with .25 sec. and once with .5 sec. exposure. The time of exposure was standardized by using a

[67]These inkblots were similar to those used by G. V. Dearborn (A study of imaginations, *Am. J. of Psychol.*, 1897, ix, 183).

[68]The observers who served in these experiments were (in alphabetical order) a graduate student, Dr. Helen Clark (C); an advanced undergraduate, Miss Helen Davis (Da); and five instructors in the department, Dr. J. E. DeCamp (De), Dr. C. Rahn (Ra), Dr. C. A. Ruckmich (Ru), Dr. A. H. Sutherland (S), and Dr. T. F. Vance (V). The writer takes this opportunity to express her sincere appreciation of their services and especially to acknowledge her indebtedness to Professor Bentley for his constructive criticism and supervision throughout the investigation.

single 'standard' card, when timing the apparatus. After the distraction-stimulus, the introspections were recorded by the experimenter, who asked such questions only as were needed to clarify the reports. The O's were all given preliminary training on four series, similar to the main series, before the twelve main series were begun.

Results. From our introspective reports we find that, for the greater part, perceptual complexes are—under our conditions—composed of sensational and imaginal materials. A few representative introspections will illustrate the type of report and the critical analysis made of the experiences:

"First there was a complex of visual sensations, which meant nothing more than an inkblot of some form. The perception of this was not very clear. Then occurred a visual image of the inkblot, very weak, but persistent. After the image, there seemed to be a break in the direction of attention. During this time there was a great deal of eye strain, which meant an endeavor to clear up the meaning. The duration was intermittent and the intensity low. There were also some kinaesthetic processes from the throat, but they were very vague and brought no definite words." (S)

"The greyness values of the blot were very clear, but meant nothing but an outline of some sort. Then came a visual image of a picture of shoes. The spot then *meant* this, and the image was assimilated right into the visual complex. Then came some 'throat' kinaesthesis and visceral sensations, meaning an endeavor to clear up the meaning." (Ru)

Affective processes were very seldom noted, and then seemed to be of minor importance for the perception, since they were usually related to some individual component rather than to the complex taken as a whole. Of the sensational processes, the visual, by virtue of their direct initiation under stimulus, played an important rôle in the total pattern, making up about 26% of the total number of processes or homogeneous groups of processes reported for all exposures by all observers. But when we find further that processes indirectly evoked within the body, the organic and kinaesthetic sensations, compose 39% of the total number, those directly evoked seem to become of relatively less primary importance so far as the 'bulk' of the perception is concerned.

When the organic sensations occurred, they were always general, but with two possible variations: first, the 'thoracic' and 'abdominal' processes were closely integrated and not distinguishable, or secondly, they were not closely fused and either one or the other stood out emphatically from the total complex. These processes by themselves, however, form but a small part (3%) of the entire group for all observers.

Kinaesthesis, on the other hand, represents the largest per-

centage (36) of any particular class of mental phenomena reported. Both muscular and tendinous sensations were observed. The muscular changes involved may be classified with respect to localization; they were either of the general, diffuse, bodily type, or specially localized in some definite part of the body. The classes of 'muscular' sensations as given in the order of frequency of occurrence are: general (474), 'verbal' (348), ocular (178), head (72), chest and face (each 3), neck (2), and hand (1). The instances of tendinous strains are all very definitely localized in the eyes and head, with a frequency of 243.

Beside the sensational processes, both directly and indirectly evoked, introspection also reveals imaginal processes of various classes, visual, auditory, kinaesthetic, and tactual. All imaginal materials considered together form about a third (35%) of all processes; the visual alone, 27%; kinaesthetic, 7%; auditory and tactual each, .6%. From these proportions, it appears that visual imagery is of importance in visual perceptions. The distribution of the various processes in the different temporal phases for all observers is given in Table I, which is composed from 882 exposures of irregular ink-blots, distributed among the five observers.

The perceptions of the 882 exposures represent 94 formations which differ among themselves as regards arrangement of processes and temporal course. Since there were 882 exposures, there were, of course, a like number of possibilities of formation. But with all these possibilities the fact that the whole number can be subsumed under 94 configurations implies that perceptions tend to conform to typical integrations. Thirteen 'typical' patterns, those with a frequency of eighteen or more, are given in Table II, where VS and VI stand for groups of visual sensations and visual images, and the subscripts F and D for the kind of meaning conveyed (*cf.* Table I and p. 548). The sign '>" stands between successive phases and the oblique lines separate parallel processes. The total number of these thirteen configurations is 460, or more than half. In Table II they have been grouped according to similarity of process, of mode of integration, and of function.

1. Group I represents integrations of visual sensations (VS), kinaesthesis, and visual imagery (VI). There is but slight variation from formation to formation, the chief difference being one of temporal sequence. General kinaesthesis stands out as one of the essential factors in each pattern. In the last three, the diffuse, bodily kinaesthesis is supplemented

TABLE I

Type of process	SENSATIONAL										
Class	VISUAL							ORGANIC			
Subclass											
Meaning of complex[1]	F	D				A	S				
Temporal phases	1	1	2	3	4	1	2	1	2	3	4 5
Total number of records of all observers	835	44	27	48	1	3	2	5	53	46	26 2
Total number of uses for each meaning	835	120				3	2				
Percentages	87	12.5				.3	.2				
Total of subclasses											
Total of class	960							132			
Percentages	25.8							3			

[1] The letters F (figurational), D (depictive), A (abstract), and S (symbolic), distinguish the perceptions according to the kind of meaning or reference which they bear (cf., p. 548, inf.).

TABLE I—*Continued*

Type of process	SENSATIONAL—*Continued*																					
Class	KINAESTHETIC																					
Subclass	MUSCULAR																		STRAIN			
	General					Chest	Hand	Head		Neck	Face	Ocular			Verbal				Eyes and Head			
Meaning of complex																						
Temporal phases	1	2	3	4	5	2	5	2	3	3	2	1	2	3	1	2	3	4	1	2	3	4
Tot. No. of Rec. of all Obs	15	300	56	43	60	3	1	70	2	2	3	37	116	25	13	202	127	6	13	150	1	79
Tot. No. uses for each meaning																						
Percentages																						
Total of subclass	474					3	1	72		2	3	178			348				243			
Total of class	1324																					
Percentages	36																					

TABLE I—*Concluded*

Type of Process	IMAGINAL																						
Class	VISUAL											AUDITORY			KINAESTHETIC						TACTUAL		
Subclass															General	Verbal	Auditory-Verbal						
Meaning of complex[1]	F			D				A		S													
Temporal phases	2	3	4	1	2	3	4	2	3	2	3	2	3	4	3	3	1	2	3	4	2	3	4
Tot. No. of Rec. of all Obs.	205	290	15	2	113	287	44	7	11	11	8	3	18	1	3	3	24	124	88	17	3	18	1
Tot. No. of uses for each meaning	510			446				18		19													
Percentages	51			45				2		2													
Total of subclasses															3	3	253						
Total of class	993											22			259						22		
P rcentages	27											.6			7						.6		

[1] The letters F (figurational), D (depictive), A (abstract), and S (symbolic), distinguish the perceptions according to the kind of meaning or reference which they bear (cf. p. 548, inf.)

TABLE II

Group	ORGANIZATION	Fre-Quency	Total for group	Percent-age
I	VS_F > General Kinaesthesis > VI_F	75		
	VS_F > General Kinaesthesis > VI_D	68		
	VS_F > Strain Sensations > VI_F > General Kinaesthesis	24		
	VS_F > Ocular Kinaesthesis > Kinaesthesis, General and Verbal > VI_F > VI_D	18		
	VS_F > Strain Sensations > $VI_F VI_D$ > Verbal Kinaesthesis > Strain Sensations > General Kinaesthesis	79	264	58
II	VS_F > Auditory-Verbal Images	28		
	VS_F > Auditory-Verbal Images > VI_F	23		
	VS_F > $VI_F VI_D$ > Auditory-Verbal Images	22		
	$VS_F > VI_F \left\{\begin{matrix} VI_D \\ VI_D \\ VI_D \end{matrix}\right. /$ Auditory-Verbal Images	20	93	20
III	VS_F > Kinaesthesis > VS_D (Head, Eyes, General, Verbal)	46	46	10
IV	VS_F > VI_F	20		
	VS_F > VI_F > VI_D	18	38	8
V	VS_F > VS_D > General Kinaesthesis (Empathy)	19	19	4
	Total		460	100

by special, localized kinaesthesis, such as ocular and verbal. The visual imagery involved differs as to cognitive elaboration. It may be simply the sustained meaning carried over from the corresponding sensational phase and representing a 'nonsense' figure ('figurational') or it may be imagery with derived and extended meaning depicting some definite 'sense' object ('depictive').

2. Group II represents formations of visual sensations and visual and auditory-verbal imagery. The greatest difference among the four patterns is one of complexity with respect to the number of processes; the second may be considered as a greater elaboration of the first, in that the second terminated with a visual reproduction of the figure, which the first perception lacked. The remaining two formations are modifications of the second in respect to temporal sequence.

3. Group III is composed of but one type of integration. It is distinctively set off from the other groups by the fact that the object itself—as carried by visual sensations—changes from being apprehended as a mere irregular figure to one possessing a high degree of meaning.

4. Group IV is characterized by its absence of kinaesthetic and organic processes. Introspection revealed nothing but visual materials, sensational and imaginal. The second formation differs from the first in degree of meaning, the first never acquiring any more significance than that of an irregular, black figure. The temporal sequence is identical, with the exception that the second expanded into a third phase, in which derived meaning appeared attached to visual imagery.

5. Group V bears a strong resemblance to Group III with respect both to composition and to the function of some of the component processes. But there are also wide differences between the two groups as regards, first, the function of other processes and, secondly, the temporal course of the perception. First, as to similarities, the complexes of both groups are composed of visual and kinaesthetic sensations. Furthermore, the sensations of both groups, which were directly evoked, bear not only the meagre meaning of an irregular figure, but also acquire other significance. As to differences, first, in regard to function, the kinaesthetic factors serve two distinct purposes, that of effort or intent to seek meaning in the formations of Group III, and that of empathic interpretation in those of Group V. Secondly, there is a variation in temporal sequence. The visual processes bearing the derived meaning came after the kinaesthesis (effort) in the combinations of Group III, but accompanied the empathic kinaesthesis in those of Group V.

The groupings of these various perceptual formations may be determined not only upon the basis of similarity of combinations of processes by all observers, but also upon the basis of typical integrative patterns for individuals. Group I is characteristic of all observers (C, 5%; Ra, 20%; Ru, 50%; S, 25%; V, 6%); Group IV, of three, Ru, (6%), S (6%), and V (4%); Group V of three, C (10%), Ra (27%), and S (8%); Group II, of two, C (11%) and V (24%); Group III, of one, S (33%). We may further say that the type of perceptual pattern included in Group I is especially characteristic of Ru; that of Group II, of V; of Group III, of S; of Group V, of Ra; while observer C displays a high frequency in both Groups II and V.

If the integrations should be combined with respect to functional properties, the first two groups would be closely related. While the processes of all but the first phase differ widely, first as to type (those of the first group being sensational, those of the second imaginal), and secondly, as to localization (those of the first being general, those of the second, special), nevertheless, the processes serve similar functions within the perceptions. Both the kinaesthetic sensations and the auditory-verbal images perform the function either of *bearing meaning* or of self-instruction to *seek meaning* in the figure. The difference in process represents merely individual variations in interpretation or significance.

An examination of Table II reveals the following facts:

1. Visual sensations alone, for the greater part, compose the first phase of the perception.
2. Kinaesthesis, general and special, is an important component within most perceptual complexes and usually occurs in the second phase.
3. With very few exceptions, imagery, especially visual, occurs in every perception, but its appearance is, for the greater part, comparatively late in the temporal course.

From Table I we see that introspection revealed seventeen different kinds of process within the perceptual complexes. Further investigation discloses the fact that these processes were distributed over no less than five temporal phases within the perceptions. Table III represents the distribution of the various processes in all the 882 perceptions with respect to kind and function, in the five temporal phases. The processes are arranged in each phase according to their order of frequency, from the greatest to the least.

Thus, in the first temporal phase of the perception, visual sensations, bearing the mere apprehension of the black figure, were the most frequent (835); the visual complex which carried a depictive significance, second in frequency (44); and so on. But parallel processes in the various phases are

TABLE III
TEMPORAL PHASES

Order	1	2	3	4	5
1	VS_F	General Kinaesthesis	VI_F	Strain Sensations	General Kinaesthesis
2	VS_D	VI_F	VI_D	VI_D	Organic Sensations
3	Ocular Kinaesthesis	Verbal Kinaesthesis	Verbal Kinaesthesis	General Kinaesthesis	Hand Kinaesthesis
4	Auditory-Verbal Imagery	Strain Sensations	Auditory-Verbal Imagery	Organic Sensations	
5	General Kinaesthesis	Auditory-Verbal Imagery	General Kinaesthesis	Auditory-Verbal Imagery	
6	{Verbal Kinaesthesis Strain Sensations	Ocular Kinaesthesis	VS_D	VI_F	
7	Organic Sensations	VI_D	Organic Sensations	Verbal Kinaesthesis	
8	VS_A	Head Kinaesthesis	Ocular Kinaesthesis	{VS_D Auditory Imagery Tactual Imagery	
9	VI_D	Organic Sensations	{Auditory Imagery Tactual Imagery		
10		VS_D	VI_A		
11		VI_S	VI_S		
12		VI_A	{General Kinaesthetic Imagery Verbal Kinaesthetic Imagery		
13		{Facial Kinaesthesis Chest Kinaesthesis Auditory Imagery Tactual Imagery	{Head Kinaesthesis Neck Kinaesthesis		
14		VS_S	Strain Sensations		

not to be interpreted as of approximately equal frequency; *e. g.,* the occurrences of general kinaesthesis (300) in the second phases are not so numerous as those of VSF, in the first. The table simply indicates the relative importance of each kind of mental phenomenon in each temporal division of the total complex. While bodily kinaesthesis occurs in each phase, it may, nevertheless, be said to be most characteristic of the second, *i. e.,* if it appears anywhere within a perceptual integration, it is more apt to be subsequent to the processes directly evoked. Table III further shows that the accessory processes in every phase but the first are of much more significance to the meaning of the perception than the concomitant, directly initiated processes. For example, while the visual sensations may in the second, third, and even the fourth phase, possess significance other than merely that of a black area on white, nevertheless various other complexes have precedence in frequency. Of course, with other materials, the perceptual value of the first phase might be greatly emphasized.

Perceptions, then, which arise from the observation of ink-blots, are composed of three fundamentally different kinds of processes; (1) those directly evoked, (2) those which are related to organic movement, and (3) imaginal materials derived from various sources. These have all been shown to be of primary importance to the perception, each discharging its own peculiar service or supplementing that of other processes.

In this investigation, the visual complexes were found to perform four various functions. First, the object was apprehended, either directly or indirectly, as being merely a black area on white.

"The visual complex simply meant an irregular figure of some sort." (Ru)

This type of meaning we shall designate as *figurational* (VSF or VIF). Secondly, the same complexes were involved in the ascription of other meaning, in depicting the figure as some particular object (VSD or VID), *depictive* function.

"The figure meant a bull-moose, horns and all very definite." (S)

Thirdly, the visual processes did not depict a particular object such as a particular hill, tree, or house, but they bore the meaning of a concept, as of centipede or of quadruped or of triangularity.

"In the perception, the object immediately became a general representation of such an animal as a centipede." (S)

We have designated this function as *abstract* (VSA and

VI A). Fourthly, the figure might be apprehended, not as some definite and particular object, but as a representation of an object, as a symbol (VSs and VIs), *symbolic function.*

"The visual and auditory imagery carried the meaning that this was a symbolic representation of a waterfall." (Ru).

Reference to Tables I and II will make it clear that in the perception of figures which possess no, or only slight meaning the visual processes more frequently display figurational and depictive functions than abstract and symbolic. While the figurational and depictive functions, taken together, occur with the same frequency in the sensational (955) as in the imaginal (956) processes, yet there is a decided difference when considered separately. As many as 835 of the visual sensation-groups were figurational and but 120 depictive, while in the case of imagery, 510 were figurational and 446 depictive, thus indicating that imagery is here of more importance than sensations in giving derived significance.

Again, in the case of abstract and symbolic functions, there is a greater frequency in imagery than in sensations. Thus, the primary function of the directly initiated processes becomes one of bare apprehension of object, while that of imaginal materials is to elaborate upon the meaning.

Whatever primary meaning the perception possessed usually appeared with the visual complexes. In this sense the perceptions are correctly called 'visual.' The kinaesthetic and organic processes, sensational and imaginal, did little at first to add to or to modify it. Their chief function in the first phase was (1) to bear effort or intent to find-significance-in-the-figure, or (2) to question the fitness of meaning ascribed by other processes. Thus, they were accessory to the visual complexes rather than directly assimilated to them. Group V of Table II, however, is an exception to this kind of function. In this mode of integration the empathic kinaesthesis substantiated the meaning attached to the visual sensations, thus actually displaying a depictive function, although the interpretative function is still evident.

The auditory and tactual imagery perform a service similar to that of the organic and kinaesthetic processes, *i. e.,* they carry self-instructions either to seek meaning in the figure or to determine the appropriateness of significance. They primarily represent individual differences in the observers in the interpretation of the figures. Auditory and auditory-verbal imagery in the cases of C and V and tactual imagery in the case of Ra discharge the same office as kinaesthetic

processes do for other observers and frequently also for these same observers.

Summarizing our experimental results thus far presented, we may say that perceptions have a tendency toward certain integral formations which possess peculiarities due to individual differences but which, nevertheless, conform in general to common patterns. Within these configurations, the various processes follow a temporal course quite definitely determined; the directly evoked sensations showing a marked precedence over all others in the first phase, the indirectly initiated sensational complexes composing the greater part of the second, and imaginal processes holding predominance in the third. Again, the various processes which compose the perception display some specificity of function; the visual processes bear the primary meaning, while those connected with organic movement together with the imaginal components both interpret and extend whatever significance the figure may derive and enforce the intent to enrich the perceptual meaning.

B. The Elaboration of Perceptual Meaning.

Problem and method. Typical integrations having been established for perceptions of figures with very slight or nominal significance, it became the primary task of this immediate part of the investigation to determine whether an essential change occurs in perception when material with a slightly higher degree of significance is used. For this purpose, we made use of hieroglyphics and cubists' drawings. A series of 50 cards was made, bearing objects which suggested various degrees of meaning. The following illustrations are typical of the series.

These cards were exposed as the ink blots were for ½ sec. to the observers, C, Ra, and Ru. The instructions were as follows:

"Two seconds after a 'ready' signal, a figure will be briefly exposed. A distraction will be given at the end of two seconds. Upon distraction, report at once whether the figure is wholly devoid of significance. Then give full introspections."

Results. No difference in composition was discovered between the perception of inkblots with minimal meaning and the perception of figures possessing a greater degree of significance.

The complexes revealed types of integration similar to those of the first set. But, although there is no difference on the

side of composition, there is a marked change in the function of the different processes. Whereas, in the perceptions reported under the first experiment, many processes bore the general function of seeking and interpreting meaning, the same processes serve other purposes as well when the object becomes more 'meaningful.' In this series of investigations we have distinguished three kinds, interpretative, appreciative, and orientating.

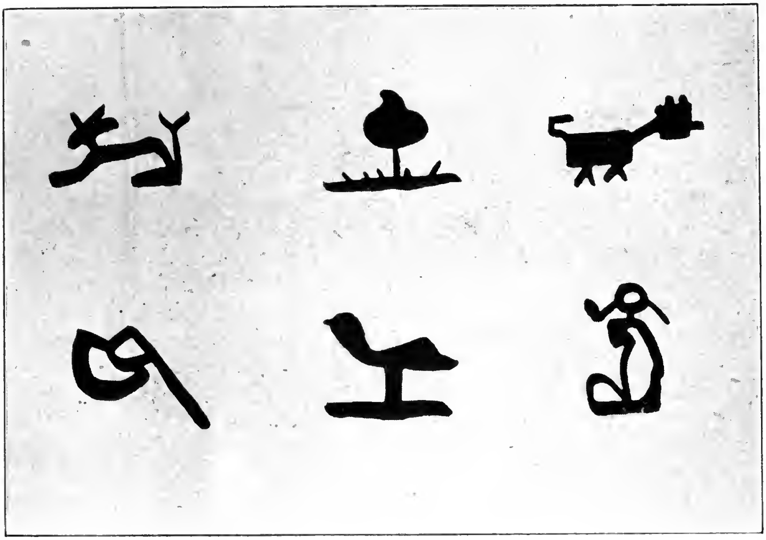

FIG. I

The interpretative function is of the same type as we found prevalent in the perceptions of the first experiment. It is the effort or intent on the part of the observer to read meaning into the object. The following excerpts will illustrate its application as related to various types and classes of mental phenomena:

"I felt myself (general empathic kinaesthesis) standing erect and rigid, and then the meaning 'grass' came." (C)

"There was a very clear auditory image of the word 'bowl' and with it a general kinaesthetic set which meant the realization that the object was not like a bowl. Kinaesthesis of the hands and tactual imagery gave 'roundness' to the top." (C)

"There was much organic disturbance, which meant that the figure was complex, that it was a problem which I must solve." (Ru)

"Visual images assimilated with visual sensations gave the meaning of solidity." (Ra)

These illustrations not only indicate a similarity to the interpretative function of the processes in Experiment 1, but also a difference. In Experiment 1 we found that the interpretation was of the desiring, seeking, searching type without any satisfactory fulfillment, *i. e.,* if derived meanings did occur as a result of the search, they were usually rejected or at least not wholly accepted. In Experiment 2 we find this same intent and desire to derive meaning but with a difference. In the first, the effort is of the kind involved in 'puzzle' consciousness; in the second, it is accompanied by satisfactory realization.

Both sensational and imaginal components commonly functionate as explorers for meaning in the object. The sensations which serve in this manner have both a limited (visual) and a general (organic and kinaesthetic) origin. In the case of imagery, the materials are drawn from various sources, specific (as visual, auditory, and olfactory) and general (as tactual and kinaesthetic). The *interpretative* function, then, is not peculiar to a particular class of mental processes, although effort itself is directly connected with kinaesthesis.

The second function we have called the *appreciative* function, a term employed to designate the service of a process or complex of processes when the figure is valued, *i. e.,* where it gives rise to pleasure, enjoyment, or aesthetic sentiments. Examples follow.

"There was a general, kinaesthetic and organic complex, meaning amusement because the dog was so fantastic and so incongruous." (C)

"There were numerous visual images of scalps and skins and of tortoise-shell combs of variegated colors. With this was a pleasant organic complex. The total complex signified the richness of meaning of the object." (Ru).

"There was a background of visual imagery, associations from the history of man, carrying an appreciation of the feeling of unity with man." (Ra).

As regards the interpretative function, we found that any kind of mental process might fill such an office, whereas only the indirectly evoked sensations (organic and kinaesthetic) and imaginal processes carry the appreciative function and, moreover, that the imaginal processes which here functionate as appreciation are limited to the two classes, visual and kinaesthetic-verbal. This would seem to indicate either (1) that the meaning of these perceptions was not yet complex or

evident enough to involve many processes in appreciation, but still demanded much interpretation, or (2) that fewer kinds of process actually perform this valuating or estimating service.

The *orientating* function is a third kind of performance distinct from either the interpretative or the appreciative. There is no effort or attempt involved to attach purport to the object, nor is there any valuation of such meaning as may be present, but the processes functionate in placing or localizing in time or space the object with reference either to the observer or to other objects.

"There were some organic processes in the background, allied with kinaesthetic processes which gave the object position." (Ru)

"There was confused visual imagery referring to sketching classes where they had drawings of friezes, of Egyptian designs, and of Greek and Roman figures." (C)

"There came an auditory image of the word 'curlique' which was accompanied by auditory imagery referring it back to other diagrams meaning the same thing." (Ru)

"This reference to books on archeology came in terms of visual imagery of pictures and kinaesthetic-tactual imagery of the feel of the page with print and pictures." (Ra)

The orientating function, then, is characteristic of organic and kinaesthetic sensations and of imaginal processes from various sources. Of the images derived from definite sources, we find visual and auditory; of those with a diffuse origin, tactual and kinaesthetic.

Although three kinds of functions may be assigned to the various processes, nevertheless, as far as our results indicate, we cannot say that all are of equal importance in giving meaning to the perception. In Table IV the total number of processes has been recorded under each function with respect to the capacity of each for carrying these three kinds of significance. The interpretative, appreciative, and orientating functions are indicated by I, A, and O, respectively. The symbols + and — indicate whether or not processes discharging particular functions, added to the significance of the perceptions,—a significance, which had already been derived, *e. g.,* the meaning 'horn' is already attached to a visual complex, when an auditory image of the word 'horn' subsequently occurs. The latter supplies nothing new to the context and is therefore set down as —.

TABLE IV

Observers		No. of + and — Processes								Total No. of Processes			
		I		A		O		A and O		I	A	O	A and O
		+	—	+	—	+	—	+	—				
C	Totals.........	152	27	21	0	42	17	63	17	179	21	59	80
Ra		77	18	8	1	15	9	23	10	95	9	24	33
Ru		96	66	35	1	57	35	92	36	162	36	92	128
C	Percentages....	59	10	8	0	16	7	24	7	69	8	23	31
Ra		60	14	6	0.8	12	7	18	7.8	74	6.8	19	25.8
Ru		33	23	12	0.3	20	12	32	12.3	56	12.3	32	44.3
	Grand totals...	325	111	64	2	114	61	178	63	436	66	175	241
	Percentages....	48	16	9	0.3	17	9	26	9.3	64	9.3	26	35.3

From an inspection of this Table we may conclude as follows:—

1. In perceptions possessing a moderate degree of meaning, processes which serve as 'interpretation' are approximately twice as frequent (436) as processes which valuate and localize (241) the object. This observation points to the fact that the meaning was not so evident but that most of the components in the perceptual process stood for effort or intent to attach significance to the figure.

2. Every kind of component in the perception shows a greater tendency to be positive rather than negative to the meaning, *i. e.*, in every instance, the processes which contributed to the meaning greatly outnumber those which failed to enlarge the significance. All of the 'appreciative' processes, with two exceptions, enriched meaning. The phenomena which localized the object, however, made no contribution to the meaning about half as frequently as they added thereto. Although we have said that processes add or fail to add meaning, we cannot say how much each process contributes to the significance of each perception. For example,—to take a typical illustration,—we perceive an irregular figure which assumes the derived meaning of 'cannon' by virtue of the visual sensations themselves. Then appears a visual image of this spot, still possessing the meaning 'cannon.' In this instance, it is evident that the sensations were of much more importance to the meaning than the images, and that although there were two types of process we cannot say that each contributed one-half the meaning. Meaning itself is 'richer,' 'poorer,' 'more or less elaborated;' but it is not divisible into fractional parts.

3. After the interpretative processes have run their course, other processes have a greater tendency to serve in placing the object in space and time than for appreciation. There are approximately three times as many 'O' as 'A' processes.

4. From the general results of Table IV, there seems to be a temporal evolution of the various functions of process from perceptions with minimal meaning to those with considerable meaning. When meaning is minimal, the processes serve for bearing and interpreting it. Then as meaning becomes more elaborate, the processes acquire other functions, first that of localization, and secondly that of appreciation.

Table IV also reveals some individual differences with respect to the functional aspect of mental processes.

1. The 'I' processes vary from 56% for Ru to 74% for Ra. (This difference should be considered in connection with Table V, below.)

2. Ru shows a higher ratio between his 'O' and 'A' processes and his 'I' processes than either C or Ra.

3. There are also marked differences with respect to the relation of the processes to the meaning. Ru has approximately as many 'A' and 'O' as 'I' processes which add to the meaning, while C and Ra show a predominance of positive 'I' processes. In Ru's perceptions, also the 'I,' 'A,' and 'O' processes which do not add to the meaning have a lower ratio to those that are positive than is true in the case either of C or of Ra.

We have seen that there is a tendency for the 'A' and 'O' processes to increase in frequency as meaning becomes more elaborate. In our next table (V) this relationship of process to degree of meaning becomes more clearly apparent. Here the meaning of the object has been designated as 'none,' if it possessed no significance other than that of a black area on white. It will be recalled that this series was purposely so made as to bear varying degrees of suggestion. 'Slight' indicates that there was present a low degree of meaning, which was usually indicated by hesitation on the part of the observer to accept it, or by lack of details, or by the predominant function of all the processes involved. 'Considerable' indicates that the object was rich in detail and setting.

In the case of 'no' meaning the results quoted in Table V show that but few processes with the orientating function occurred and none of the appreciative type. But in perceptions, which possess 'slight' meaning, there is a greater frequency both of the 'A' and 'O' kinds of process with a greater prevalence of the 'O' type. The same is true of complexes listed under the third degree of meaning. This further substantiates the statement made above that as meaning develops more and more the accessory components of the perception acquire new functions, namely, the orientating and the appreciative. The processes discharging these services also vary directly in frequency with the significance. Table V further shows that, if judged by frequency, the same classes of process assume the orientating function before the appreciative, *i. e.,* in the instances of 'no' meaning, there were but few 'O' processes, and no 'A' processes, while in the other instances, the 'O' processes always exceed the 'A's'. This fact illuminates the reason for Ru's displaying such a high frequency of 'A' and 'O' processes (Table IV). From Table V, we see that Ru reported no perception under the 'none' degree of meaning.

These facts accord with those of the first series of experiments, where, with the exception of the integrations listed under Group V of Table II, all accessory processes presented

TABLE V

Observers		Meaning											
		None				Slight				Considerable			
		I	A	O	A and O	I	A	O	A and O	I	A	O	A and O
C	Totals......	12	0	2	2	58	5	18	23	109	16	39	55
Ra		7	0	3	3	26	2	7	9	62	7	14	21
Ru		0	0	0	0	48	8	27	35	114	28	65	93
C	Percentages	5	0	.8	.8	22	2	7	9	42	6	15	21
Ra		5	0	.2	.2	20	2	5	7	49	5	11	16
Ru		0	0	0	0	17	3	9	12	39	10	22	32
	Grand totals	19	0	5	5	132	15	52	67	285	51	118	169
	Percentages	3	0	.7	.7	19	2	8	10	41	8	18	26

clearly the interpretative function, since the figures used in those experiments possessed just as little meaning as possible. In the case of perceptions which involved empathic kinaesthesis, which was mostly interpretative, but with a suggestion of the localizing function, the objects were probably of the same degree of meaning for the various observers as those judged as having "no" meaning in the second series of 'object' cards.

The following general conclusions may be drawn from the results of Experiment 2:

1. The accessory processes, kinaesthetic and organic sensations and various imaginal components, acquire functions other than the interpretative, as meaning grows more elaborate. In this experiment these new services have been found to be appreciative and orientating.
2. The accessory processes which perform the appreciative and orientating functions increase in frequency directly with the degree of meaning.
3. The various functions of mental processes appear in a definite order, in perceptions of varying degrees of meaning; (1) interpretative, (2) orientating, and (3) appreciative.

C. Relation of Attention to Perception.

Up to this point our experimental results have shown us, first, that perceptions fall into typical integrations, secondly, that perceptions present various degrees of cognitive and appreciate elaboration, and thirdly, that the elaboration of significance is relatively independent of the particular kind of mental processes involved. But our analytic problem is not yet complete. We must not neglect the state and the configuration of the total bit of mind in which the perceptual complex is embedded. This is the problem of the relation of attention to perception. We approach it by taking an inventory of all the processes, at their several degrees of clearness, which lie both within and without the particular constellation which we have undertaken to study.

1. *Preliminary series.*

Problem and method. In order to train our subjects to observe these degrees of clearness or vividness, two series of preliminary experiments with distraction were performed. In the first a series of nonsense-syllables was exposed, approximately one each second, which was to be memorized by the observer. Some time during the learning-period a distraction was offered by drawing a pattern upon the back of O's left hand with the blunt end of a pen shaft. At a signal "now,"

O introspected for that instant, estimating the various processes according to nine degrees[69] of clearness.

The second series of experiments consisted of a double task. O was given one of the cancellation test-sheets and asked to mark all 'a's'. While he was thus engaged, simple arithmetical sums and multiplications were orally given and their solution demanded. At a signal 'now' full introspections were reported with respect to the clearness of the processes, which were present just at the signal 'now.' The experimenter in both series always attempted to give the signal 'now' while the observers were distracted either by the pattern or by the computation.

The instructions for the first series, where C, De, Ra, and Ru observed, read as follows:

"A 'ready' signal will be given. Two seconds later, the first of a series of nonsense-syllables will be exposed. Learn the series. During the learning-period, a distraction will be given by drawing a pattern on the back of your hand. At a signal 'now,' give an introspective account of the degrees of clearness of all processes, present at the signal 'now.' The degrees of clearness are to be judged in the following terms:

maximally clear	1
very clear	2
clear	3
fairly clear	4
fair	5
fairly vague	6
vague	7
very vague	8
obscure	9"

Instructions for the second series were:

"A 'ready' signal will be given. Two seconds later a signal 'start,' at which begin crossing out the "a's" on the sheet before you. During this process, simple arithmetical additions and multiplications will be given for about two seconds. At a signal 'now,' give an introspective account of the degrees of clearness of all the processes present just at the signal 'now.' The degrees of clearness are to be judged in the following terms; [the same 9 degrees were used as for the first series]."

Results. The two preliminary series of experiments resulted not only in training the O's in the use of the degrees of clearness, but also determined the type of consciousness of each observer, whether it was of the foreground-background type or multilevel. Graphs I-IV, showing the results for nonsense-syllables, illustrate individual differences.

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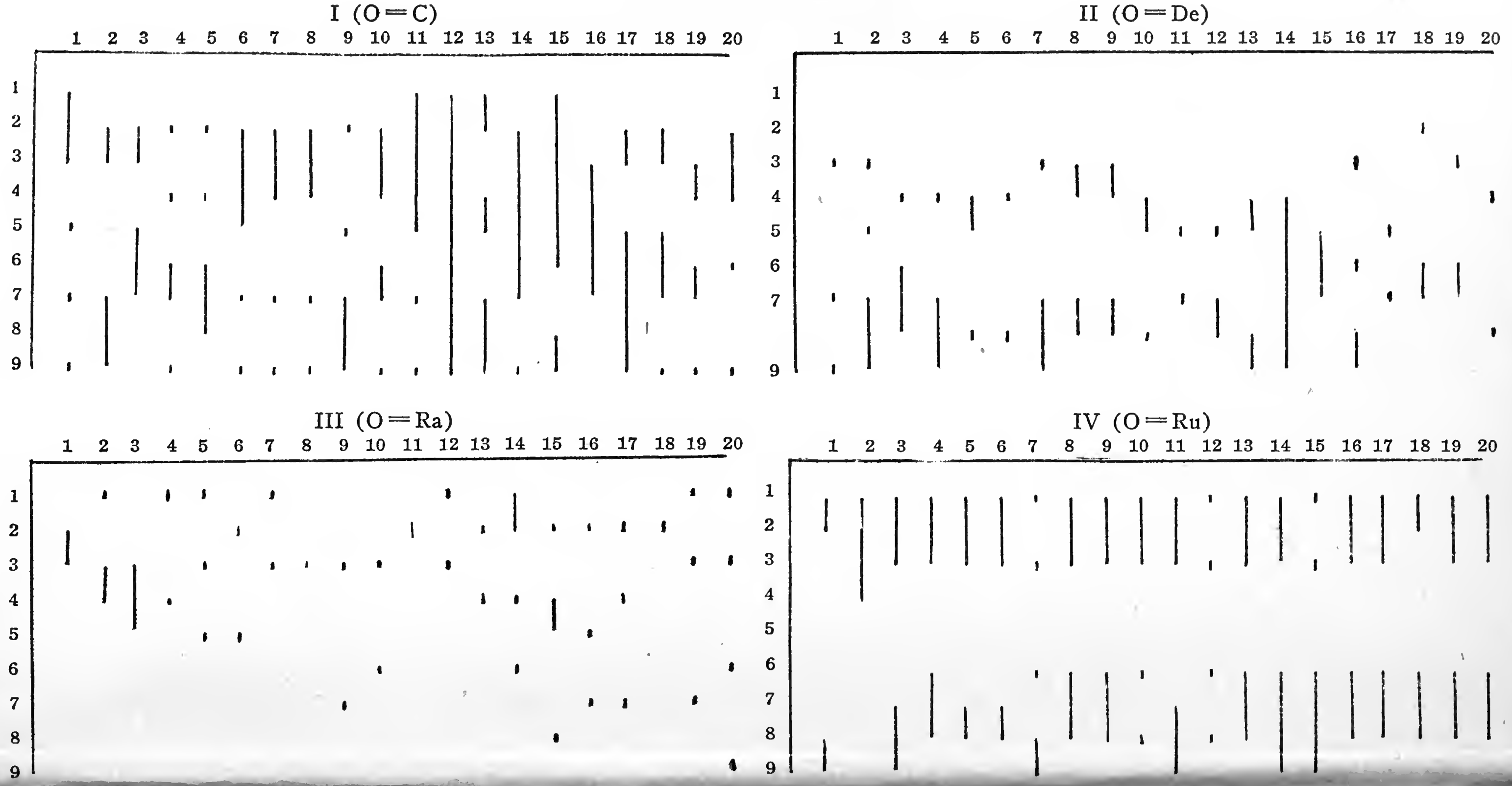
[69]We employed the same 9 values for determining the degrees of attention as were used by Geissler, (The measurement of attention, *Am. J. of Psychol.,* 1909, xx, 502) and Dallenbach, (*ibid.,* 1913, xxiv, 468).

DEGREES OF CLEARNESS



In the graphs the trials are ordered on the abscissae, the degrees of clearness on the ordinates. Adjoining degrees are connected by solid vertical lines.

The diagrams then suggest that C and Ra and possibly also De, represent multilevel types of mind. The data for the double task series indicate results similar to those for the first series. Both are included in Table VI. The symbols 'N-S' and 'A' are employed to represent the nonsense-syllables and the cancellation sheet, respectively. It may be said in general that the observers exhibit a similar distribution of processes among the nine degrees of clearness with the exception of Ru, whose two-level division reduces the numbers for the medium and the very lowest degrees. And again, when Ru's estimations of vividness are divided into three large groups (for the sake of inspection), there is practically an equal distribution for each group. Ra, while he grouped most processes in the middle level for the 'N-S' series, nevertheless exhibits a greater frequency among the maximally clear than among the obscure processes. In his 'A' series, very clear processes are more frequent than those which are moderately clear, with still fewer obscure components. The general tendency for all observers seems to indicate that most factors within the perceptual complexes are moderately clear, fewer are obscure, and a still smaller number maximally clear.

2. *Main series.*

Problem and Method. The primary task of this investigation was to estimate the bearing of clearness upon perception. To this end, the original series of inkblots (see Experiment 1) was again used. As before, the observer gave full introspective reports concerning the perception, but, in addition, the clearness of each process was given on the scale of 1-9 according to the method employed with nonsense-syllables and the literal tests of our preliminaries.

The instructions read:

"Signal, exposure and distraction-stimulus will be given as before. During the whole period of observation take the perception quite passively; do not force an 'object.' Give full introspections, indicating perceptions and other meanings in parallel wth the description of processes. The introspections should include (a) analysis of complexes, (b) the sequence and order of groups, and (c) the area of maximal clearness. The degrees of clearness are to be judged in the following terms: [See instructions for preliminary series]."

The time of exposure was ½ sec., but introspection covered a period of 2 sec. C, De, Ra, and Ru observed.

TABLE VI

Clearness values	9 Degrees															
	C				De				Ra				Ru			
	N-S		A		N-S		A		N-S		A		N-S		A	
	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%
1	5	3	1	.4	0	0	0	0	15	17	17	30	30	18	18	14
2	18	14	17	8	10	6	0	0	19	22	10	18	25	15	20	16
3	18	14	28	13	31	19	9	7	14	16	6	11	28	16	19	15
4	16	12	37	17	20	12	35	26	10	12	4	7	2	1	0	0
5	14	11	27	13	23	14	22	16	8	9	8	14	2	1	14	11
6	14	11	26	12	24	15	12	9	6	7	3	5	24	14	20	16
7	19	15	23	10.6	27	17	21	16	10	12	3	5	22	13	17	13
8	7	5	27	13	19	12	29	22	3	4	2	3	30	18	19	15
9	19	15	27	13	8	5	6	4	1	1	4	7	7	4	0	0

Clearness values	3 Degrees															
	C				De				Ra				Ru			
	N-S		A		N-S		A		N-S		A		N-S		A	
	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%
1	23	18	18	9	10	6	0	0	34	40	27	47	55	32	38	30
2																
3																
4	62	48	118	55	98	61	78	58	38	44	21	37	56	33	53	42
5																
6																
7																
8	45	34	77	36	54	33	56	42	14	16	9	16	59	35	36	28
9																

TABLE VII (O=C)

Clearness values....	1			2			3			4			5			6			7			8			9			All values			%			Proc. in 3 phases	%	Type of Proc.	%	Class of Proc.	%
Phases............	1	2	3	1	2	3	1	2	3	1	2	3	1	2	3	1	2	3	1	2	3	1	2	3	1	2	3	1	2	3	1	2	3						
Processes																																							
Visual............																																						207	37.3
Sensations.......	4			21	11		27	10		20	6	1	4	4		5	2		4			1		1	2			88	33	2	16	6	.4	123	22	123	22		
Images...........																																				84	15		
Spot.						1					1	1		1			8	1		7	3	1	5	5		2	7	1	24	18	.2	4	3	43	8				
Associations ...						1					3			2	2		2	6		6	9	1	1	5		3		1	17	23	.2	3	4	41	7				
Auditory images....					1	3		8	10		7	5		7	1		7	2	2	3	1		5	3		1		2	39	25	.4	7	5	66	12	66	12	66	12
Cutaneous images. .																																				2	.4	2	.4
Pain..........																								2						2			.4	2	.4				
Organic sensations..														1		1						1	1	3		2	2	2	4	5	.4	.7	.9	11	2	11	2	11	2
Kinaesthesis.																																						268	48.3
Sensations..																																				231	42		
Muscular:																																							
General.				2					3	3				6	3	9	6	1	27	10	3	22	18	13	18	18	9	81	58	32	15	10	6	171	31				
Legs.......																1												1			.2			1	.2				
Arms.......										1		1				1	2		4	5	1	1	6	1	2	1	2	9	14	5	1	2	.9	28	5				
Face.......												1			1									3						5			.9	5	.9				
Eye.......																							1		1			1	1		.2	.2		2	.4				
Throat......																			1	2		1		2				2	2	2	.4	.4	.4	6	1				
Strain:																																							
Neck.......				1								1				1		1	2				1					4	1	2	.7	.2	.4	7	1.3				
Face.......																						1						1			.2			1	.2				
Eye.......				1								1				1		1	3				1					5	1	2	.9	.2	.4	8	1.4				
Throat......											1										1								1	1		.2	.2	2	.4				
Images.........																																				37	6.6		
General.......																	1			1									2			.4		2	.4				
Aud.-verbal....		2	1		6	6		6	6		4				2						1						1		18	17		3	3	35	6.3				
Clrns. vals. by phases	4	2	1	25	18	11	27	24	19	24	22	11	4	21	9	19	28	12	43	34	19	29	39	38	23	27	21	198	215	141									
All phases..........	7			54			70			57			34			59			96			106			71			554											
Percentages........	1			10			13			10			6			11			17			19			13			36	39	25									
Values in 3 degrees.	61						220												273																				
Percentages........	11						40												49																				

TABLE VIII (O = De)

Clearness values....	1			2			3			4			5			6			7			8			9			All Values			%			Proc. in 3 phases	%	Type of Proc.	%	Class of Proc.	%
Phases............	1	2	3	1	2	3	1	2	3	1	2	3	1	2	3	1	2	3	1	2	3	1	2	3	1	2	3	1	2	3	1	2	3						
Processes																																							
Visual............																																						101	64
Sensations.......							10			11	1	..	20			11			5			2			1			60	1		38	.6		61	39	61	39		
Images..........																																				38	24		
Spot..........									1		2			2	2		3	1		3	2	4	9	4	1	4	2	5	23	12	4	15	7	38	24				
Kinaesthesis.																																						57	36
Sensations.......																																				5	4		
General........																						3	1					3	1		2	.6		4	3				
Arms..........																							1						1			.6		1	.6				
Images..........																																				52	33		
Verbal........				1	1	1	4	2	1	7	4		6	1	1	7		1	5	1		3	4	1				33	13	5	21	8	4	51	32				
General........																				1									1			.6		1	.6				
Clrns. vals. by phases	..	..	..	1	1	1	14	2	2	18	7	..	26	3	3	18	3	2	10	5	2	12	15	5	2	4	2	101	40	17									
All phases..........					3			18			25			32			23			17			32			8			158										
Percentages........					2			11			16			20			15			11			20			5		64	25	11									
Values in 3 degrees.			3											98									57																
Percentages........			2											62									36																

TABLE IX (O = Ra)

Clearness values	1			2			3			4			5			6			7			8			9			All Values			%			Proc. in 3 phases	%	Type of Proc.	%	Class of Proc.	%
Phases	1	2	3	1	2	3	1	2	3	1	2	3	1	2	3	1	2	3	1	2	3	1	2	3	1	2	3	1	2	3	1	2	3						
Processes																																							
Visual																																						134	59
Sensations	16	1		20	8	1	6	6	1	6	6		5	7	3	3	3	2	3	2		1	1	1				60	34	8	26	15	4	102	45	102	45		
Images																																				32	14		
Spot					2			2							1		1			1									6	1		3	.4	7	3				
Associations		1		2	6		4	2	3		1			1			2	1		1	1							6	14	5	3	6	2	25	11				
Auditory Images					1			1		1							1											1	3		.4	1		4	2	4	2	4	2
Cutaneous images																																						19	8
Pressure	2	1		4	4				1	3	2						2											9	9	1	4	4	.4	19	8	19	8		
Organic sensations		1			1			1			2		1	1														1	6		.4	3		7	3	7	3	7	3
Kinaesthesis																																						64	28
Sensations																																				24	10		
Muscular:																																							
General				2	2			1									1		2			1						5	4		2	2		9	4				
Shoulders	1																1											1	1		.4	.4		2	.8				
Arm	1						2							1														3	1		1	.4		4	2				
Head	1				1												1		1									2	2		.8	.8		4	2				
Eye								1		2																		2	1		.8	.4		3	1				
Forehead								1								1												1	1		.4	.4		2	.8				
Images																																				40	18		
General	3	2		5	3	1	3	2	1	2	2		2			1	3		1	1					1			18	13	2	8	6	.8	33	14				
Hand				1	2					1	1			1														2	4		.8	2		6	3				
Aud.-verbal				1																								1			.4			1	.4				
Clrns. vals. by phases	24	6		35	30	2	15	16	6	15	14		8	11	4	5	15	3	7	5	1	2	1	1	1			112	99	17									
All phases	30			67			38			29			23			23			13			4			1			228											
Percentages	13			29			16.6			13			10			10			6			2			.4			49	43	8									
Values in 3 degrees	97						113												18																				
Percentages	42						50												8																				

TABLE X (O = Ru)

Clearness values	1			2			3			4			5			6			7			8			9			All Values			%			Proc. in 3 phases	%	Type of Proc.	%	Class of Proc.	%
Phases	1	2	3	1	2	3	1	2	3	1	2	3	1	2	3	1	2	3	1	2	3	1	2	3	1	2	3	1	2	3	1	2	3						
Processes																																							
Visual																																						261	36
Sensations	77	20	3	9	8	1	2	5	1																			88	33	5	12	4	.7	126	17	126	17		
Images																																				135	19		
Spot		1		3	7	1	3	3	2												1							6	11	4	.8	2	.5	21	3				
Associations		10	13	23	39	7	6	7	5								1	3										29	57	28	4	8	4	114	16				
Organic sensations							2	5	3				30	29	12	8	9	4	13	3	2	5	6	1				58	52	22	8	7	3	132	18	132	18	132	18
Kinaesthesis																																						338	46
Sensations																																				288	39		
Muscular:																																							
General								2					1			2	4		4	3		3	2	1				10	11	1	1	2	.1	22	3				
Shoulder														2															2			.3		2	.3				
Eye			1	7	2	2	4	7					2	2	1		2			1	1							14	14	4	2	2	.5	32	4				
Verbal				6	9	8	33	37	14					3	2						2		1					39	50	26	5	7	4	115	16				
Strain:																																							
General									1				1	1	1				1			1	1	1				3	2	3	.4	.3	.4	8	1				
Head				6	7	1	18	6	1	1	2		6	8	3	6	4	3	1	3	3	2	2	1				40	32	12	5	4	2	84	11				
Forehead					1			1					2	2	1	2	3	1										4	7	2	.5	1	.3	13	2				
Eye					1		3	2	2				1	1				1		1								4	5	3	.5	.7	.4	12	2				
Images																																				50	7		
Verbal	1				2	1	18	17	6				1	2		1							1					21	22	7	3	3	1	50	7				
Clrns. vals. by phases	78	31	17	54	76	21	89	92	35	1	2	..	44	50	20	19	23	12	20	11	8	11	13	4	..	..	..	316	298	117									
All phases	126			151			216			3			114			54			39			28						731											
Percentages	17			21			30			.4			16			7			5			4						43	41	16									
Values in 3 degrees	277						387												67																				
Percentages	38						53												9																				

Results. Tables VII-X present a tabulation by observers with respect to kind of process, degree of clearness, and phase of perception. They include only processes relevant to the perception and not those of the total mind.

When the various kinds of process are considered with regard to their frequency in the nine classes, wide individual variations in relative clearness appear; but they largely disappear again when the nine degrees are reduced to three.

For example, we find upon inspection of the Tables that the visual sensory complexes occurred fairly well distributed under the nine values for C and Ra, while they all belonged to Ru's area of maximal clarity, and with few exceptions in the range of moderately clear components for De. And again, with reference to the kinaesthetic complexes, which are common factors for all observers, there is a decided tendency for them to be of very low level in the case of C, but of moderate clearness for all others. The extent of distribution does seem to bear some relation to the predominant type of process, *e. g.*, C, De, and Ra, who reported visual sensations under at least seven of the nine categories, possess a predominance of visual sensations over visual imaginal complexes, while Ru, who reported no visual sensations of less than degree 3, shows a slightly higher percentage of visual imagery than of sensational material. But no fixed relation as regards relative clearness of sensation and image of the kinaesthetic kind can be made out, possibly because these processes are all alike—as some writers contend—sensational in character. Again, no constant relation obtains between the clearness of sensations directly evoked and the frequency and clearness of those indirectly initiated. For example, C, whose visual processes range from 1 to 9 degrees in clearness, reveals a higher percentage of kinaesthesis, while Ru, who reported visual processes only as of the highest degrees, also possesses a much higher percentage (39) of kinaesthetic than of visual sensations (17). Neither is there apparent a direct relationship between the vividness of visual and of kinaesthetic sensations. In C's case, where the visual processes were widely distributed, kinaesthesis showed a tendency to run very low in clearness, whereas in the case of Ru, where the visual processes were concentrated at a high level, the kinaesthetic sensations were distributed, for the greater part, over the middle values. The directly and indirectly initiated sensations then seem to vary independently of each other both as to frequency and to clearness.

If now we consider all degrees of clearness under three large groups, maximally clear, moderately clear, and obscure, a grouping which our preliminary experiments seem to justify, then these wide irregularities in large measure disappear. The difference is revealed in Table XI which brings together the footings of the individual results (in Tables VII-X). The highest frequency (49%) falls within the field of moderate clearness, while the other regions are, in the totals, virtually the same (26% and 25%). All observers agree in the large number of processes of moderate clearness; though they differ

in distribution between the two extremes of clarity and obscurity.

TABLE XI

Obs.		Clearness Values									Totals For All Values		
											Phases		
		1	2	3	4	5	6	7	8	9	1	2	3
C	Totals.........	7	54	70	57	34	59	96	106	71	198	215	141
De		0	3	18	25	32	23	17	32	8	101	40	17
Ra		30	67	38	29	23	23	13	4	1	112	99	17
Ru		126	151	216	3	114	54	39	28	0	316	298	117
C	Percentages....	1	10	13	10	6	11	17	19	13	36	39	25
De		0	2	11	16	20	15	11	20	5	64	25	11
Ra		13	29	16.6	13	10	10	6	2	.4	49	43	8
Ru		17	21	30	.4	16	7	5	4	0	43	41	16
	Grand totals...	163	275	342	114	203	159	165	170	80	727	652	292
	Percentages....	10	16	20	7	12	10	10	10	5	44	39	17
	Totals in 3 levels	438		818				415					
	Percentages....	26		49				25					

So much for the organization of the perceptions in cross-section; let us also regard the temporal course. We recall from Experiment 1 that processes indirectly evoked compose, almost exclusively, every phase but the first. Furthermore, the results of Experiment 3 show that these indirectly initiated processes are generally less clear than the visual sensations, which, in a large measure, occupy the focus of attention. Thus the perception temporally undergoes a reorganization not only with respect to kind of process but also in regard to the degree of clarity of the various components. In the first phase are usually to be found only visual sensations, but of maximal clearness, while in the second there are, as a rule, accessory processes, but standing at a lower level of clearness. This fact seems to indicate a decrease in clearness from phase to phase of the perceptual complex. In Table XII we have exhibited the relation of clearness between the processes of the first and second, and of the second and third phases of perception.

In order to maintain a constant standard, the changes were determined by the relation of the highest clearness values assigned in the two phases under comparison, *e. g.*, if both the first and second phases possessed processes of the first

TABLE XII

Observers	1st to 2nd Phase			2nd to 3rd Phase		
	—	+	O	—	+	O
C..........	50	25	13	29	19	6
De..........	27	7	4	8	6	1
Ra..........	29	13	9	8	2	1
Ru..........	53	6	30	26	16	17
Totals	159	51	56	71	43	25

degree, then they were considered as having undergone no change, but if the first phase contained a process of degree 1, and the highest of the second phase was of degree 2, it was considered as a decrease in clearness. C, for instance, out of 88 perceptions reported 50 decreases in clearness between the first and second phases, 25 increases, and 13 wherein there were no changes. The symbols —, +, and *O* signify a decrease, increase, or no change, respectively.

Table XII shows, then, that there is always a greater frequency of decreases in clearness than of increases or persistencies. This observation indicates that perception undergoes a change in clearness from phase to phase and usually a decline.

But the perceptual complex suffers not only with respect to clearness from phase to phase, but also in number of components. Inspection of the last three columns of Table XI will make it evident that, with the exception of C, every phase is less rich in processes than the one preceding. Thus perception declines both in clearness and in complexity of component members.

From this investigation we may draw the following conclusions:

1. The various areas of clearness within the perceptual complex, viewed in cross-section, differ as to the frequency of processes. The region of moderate clarity (degrees 3-6) comprises the greatest number of components, while to the perception the areas of focal distinctness and obscurity contribute unequal shares of the remainder. There were many more processes, however, in the obscure background which were wholly irrelevant to the perception and which have therefore not been included.

2. In perception, each large region of clearness also shows a predominance of some particular kind of process. The directly evoked processes lie, for the greater part, within the area of maximal clearness, while those indirectly evoked are usually moderately clear, and less frequently obscure.

3. There is usually a gradual decrease in the clearness and frequency of the various processes from phase to phase.

Nevertheless, just as we found in Experiment 2 that, although processes varied as to function, we could not decide on the basis of clearness and frequency alone which component was of the most importance to the perception, so in this experiment we cannot say whether processes with lower frequency but of greater clearness (directly evoked processes) are *ipso facto* of more importance to the perception than processes with a greater frequency and lower degree of clearness (indirectly evoked processes).

D. A Study of the Principal and Accessory Processes in Perception.

Problem and method. As a result of our experiments with objects possessing minimal or only slight significance, we have found that perceptions are composed of typical processes which display particular functions and which vary as to frequency and clearness. The processes are divided into those controlled by stimulus or directly evoked,—in our case the light sensations,—and those indirectly initiated, the kinaesthetic, organic, and imaginal processes. In this part of the investigation we have attempted to determine the relative value of the controlled and uncontrolled processes to the perception of highly significant objects. For this purpose a series of fifty miniature objects[70] was prepared and attached to cards, in order that they might be exposed in the tachistoscope. The objects were purposely chosen to call forth special imagery and kinaesthetic processes. We wished to find out whether the sensations immediately initiated, the imaginal processes, or the processes connected with organic movement bear the chief importance in the perception. According to our purpose, some of the objects were chosen for the possible initiation of auditory imagery (*e. g.,* the bell and whistle), others, for kinaesthesis (*e. g.,* the Indian clubs and hammer), others again (*e. g.,* the rose and sweet peas) to call forth olfactory images.

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[70]The series given in the order of presentation included cloves, piece of rubber sponge, match and match-box lid, pickle, bell, burned cigarette, velvet, ginger-snap, scales, whistle, date, Indian clubs, chain, wet chamois, cherry and leaf (artificial), tennis racket, bottle of green liquid, hammer, loaf sugar, lantern with red glass, thumb-tack, rubber ball, coffee grains, slice of lemon, cannon, violets, sand-paper, ball-bat, striped candy, telephone, sweet peas, fur, suit-case, orange peel, firecracker, red rose, ice, dumb-bell, half of English walnut, peanut, slice of onion, red hot wire (heated by electric current), key, chocolate candy, stamp, celery, thorny twig, screw, slice of apple, and pitcher.

These objects were exposed for one second, and introspection was demanded for just the exposure period. The observers were Da, Ra, and Ru.[5] The instructions were:

"Two seconds after a 'ready' signal, an object will be briefly exposed. Observe the object carefully. Give a full introspective account of the (1) meanings, (2) processes, sensational and imaginal, reporting upon their clearness as 'very clear,' 'moderately clear,' or 'very obscure,' and (3) temporal course of the perception."

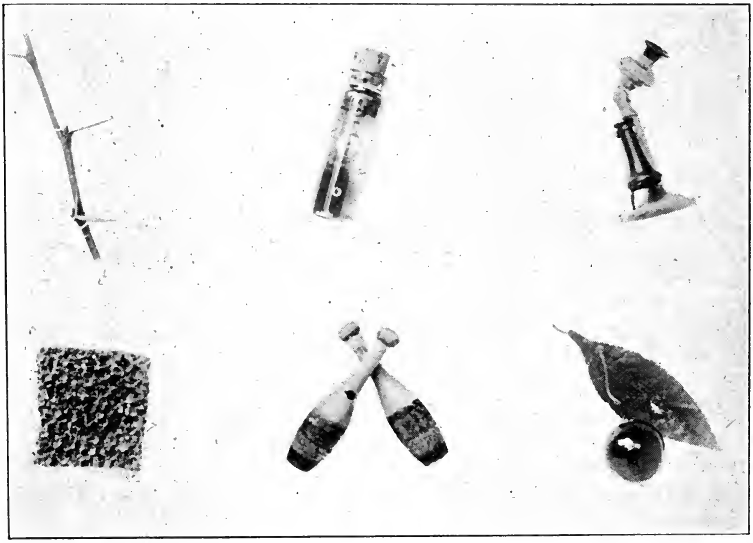

FIG. 2

Results. Table XIII gives a summary of the results of this experiment with respect to the frequency of the directly and indirectly initiated processes. 'P' and 'C' represent respectively peripheral and central processes, and '+' and '—' indicate whether or not a process or a complex was taken by the observer to add to the meaning of the perception.

The figures in the table make it evident (1) that the actual number of all kinds of accessory processes is about the same for perceptions of little or of much meaning (totals 73% and 76%); but (2) that the number of accessory processes *which are relevant and contributory to the perception* are much greater with elaborate (59%) than with slight (32%) meaning. (3) Where the meaning is considerable both the peripheral (36%) and the central (23%) accessories make

TABLE XIII

Obs.		Meaning Slight					Meaning Considerable				
		Direct	Indirect				Direct	Indirect			
			P		C			P		C	
		+	+	—	+	—	+	+	—	+	—
Da	Totals......	8	9	**12**	3	3	42	**75**	32	**43**	17
Ra		12	1	2	**9**	3	45	19	3	**46**	7
Ru		5	6	**15**	2	2	45	**107**	25	**39**	8
Da	Percentages.	23	25	**36**	8	8	20	**36**	15	**21**	8
Ra		44	4	8	**33**	11	38	**16**	2	**38**	6
Ru		16	20	**50**	7	7	20	**48**	11	**17**	4
	Grand totals	25	16	29	14	8	132	201	60	128	32
	Percentages.	27	17	32	15	9	24	36	11	23	6

large contributions to the meaning of the perception. (4) The greatest modification of function appears with the accessory peripheral (organic and kinaesthetic processes) which principally go into intent and effort where perceptual meaning is slight but are incorporated within the perception itself where objects are more richly clothed with properties and relations. (5) A notable individual difference in the perceptual use of indirectly evoked processes appears in the fact that in the cases of Da and Ru the relevant peripheral accessories greatly exceed the direct (visual) complexes where the meaning is rich (for Da 36% and 20%; for Ru 48% and 20%), whereas in the case of Ra the central processes make the greatest contribution (38%).

The relative importance to the perception of the various processes may be considered from still another point of view,—that of attentional clearness. An inspection of Table XIV shows[71] that the visual sensations present the highest frequency in the focus, while the imaginal, organic and kinaesthetic processes are variously distributed throughout all three levels.

Certain differences among the observers must be considered.

Da and Ru are again alike in keeping their central or imaginal processes at a moderate or low degree of clarity, whatever the extent of meaning, (only 3% and 7% for 'very clear') and unlike Ra whose 'very clear' central processes stand at 33% and 27%. We also saw a moment ago that Ra's central processes are relatively numerous and

[71]The symbols c, m, and o represent the various levels of clearness: very clear, moderately clear, and very obscure.

TABLE XIV

Obs.		Meaning Slight									Meaning Considerable								
		Direct			Indirect						Direct			Indirect					
					P			C						P			C		
		C	M	O	C	M	O	C	M	O	C	M	O	C	M	O	C	M	O
Da	Totals......	7	1	0	9	6	6	1	3	2	30	12	0	38	43	26	10	19	31
Ra		8	2	2	2	0	1	9	0	3	34	7	4	6	7	9	33	14	6
Ru		5	0	0	1	14	6	0	3	1	43	2	0	12	63	57	6	29	12
Da	Percentages.	20	3	0	36	17	17	3	8	6	14	6	0	18	21	12	5	9	15
Ra		30	7	7	7	0	4	33	0	11	28	6	3	5	6	8	27	12	5
Ru		17	0	0	3	47	20	0	10	3	19	1	0	6	28	25	2	13	6
	Grand totals.	20	3	2	12	20	13	10	6	6	107	21	4	56	113	92	49	62	49
	Percentages.	22	3	2	13	22	14	11	6	6	19	4	1	10	20	17	9	11	9

that they make a greater contribution to the perception than do similar processes of the other observers (Table XIII). On the other hand, Ra's organic and kinaesthetic sensations (peripheral accessories) which are fairly meagre show no decided tendency to seek a given level, whereas Ru's are chiefly 'moderate' or 'very obscure' (140 out of 153) and Da's chiefly 'moderate' or 'very clear' (96 out of 128). There seems to be no fixed relation between the clearness of these organic and kinaesthetic sensations and the contribution which they make to the perception, though this matter requires further experimental inquiry.

The results of this investigation indicate then (1) that although the sensations directly initiated compose but approximately 25% of all processes, nevertheless they are of fundamental importance to perception, since they are generally the clearest members and always bear some positive relation to the development of the meaning; (2) that the relative values of the indirectly evoked processes depend primarily upon the observer, both in respect to number, to enhancement of meaning, and to the clearness of the various processes.

There is still another very important question related to the analysis of perception which arises and may be answered from these results; namely, the absolute necessity of organic movement for perception. Each O reported the analysis of fifty perceptual complexes during this investigation. Out of that number, Da reported 10% and Ra 30% which involved no kinaesthesis or organic complexes of any type, sensational or imaginal. Ru discovered some symptom of bodily movement, general or localized, during every trial, although more than a fourth of the peripheral accessories were irrelevant to the perception. The fact that the observers reported complexes in

which such factors were either not present or were not involved indicates that the processes are not a necessity to the perception of objects. This conclusion is further substantiated by the results of Experiment 1, (Table II, Group IV) where over 8% of the perceptual formations brought under the 13 types lacked kinaesthetic or organic factors.[72] We may believe that in these and similar cases the perceptual meaning is simply and directly borne by the visual sensations and possibly the central accessories.

Chapter III

SUMMARY AND CONCLUSIONS

From the combined results of all our experiments, we may conclude that 'visual' perceptions tend, under our conditions, to present typical formations, which are composed of similar processes or members and are similarly organized or constituted. During the four experiments we have analyzed approximately 1500 perceptual complexes, all of which have revealed forms of integration the same as, or similar to, the standardized forms symbolized in Table II. We find the same component processes reported from perception to perception, whether the perception carries fragmentary, limited, or elaborate meaning. Again, we have found a progressive change in function of the various accessory processes coincident with the modification and the expansion of meaning. This modification will be made evident by a review of Experiments 1, 2, and 4 (3 used the devices of 1), where there is a gradual evolution in complexity of the materials used. The functional modifications seem to follow a definite, temporal sequence; (1) in perceptions with very little meaning, the accessory processes carry intent and they also serve to interpret the primary meaning as a 'nonsense' figure or blot, (2) in perceptions with more meaning, these processes not only interpret, but also aid in specifying and localizing the object and occasionally they are involved in appreciation, and (3) in perceptions with elaborate meanings, the specifying, localizing, and, especially, the appreciative functions become more and more frequent and elaborate. Finally, a general conclusion may be reached as regards the relative value to the meaning of the perception of the 'direct' and the 'indi-

[72] The results of a minor problem (conducted by Miss Davis, one of our observers) where the emphasis is laid upon this particular phase of perception seem further to support our facts. Three observers have thus far reported 44%, 21%, and 4% of their perceptions to be wanting in these peripheral accessory processes.

rect' processes. In all the experiments we have found a predominance of kinaesthetic and organic factors over visual sensations and imaginal processes. But, in spite of the predominance of kinaesthesis in the total number of perceptions, there have been reported in each investigation perceptions which did not involve any symptom of organic movement. The experiments indicate, then, that, although the results, sensational or imaginal, of organic movement are incorporated into many perceptual complexes, their frequent absence goes to show that objects may be perceived without their interposition.

Our results agree in general with the view maintained by several writers,—James, Bagley, Wallaschek, Lehmann, and Titchener,[73]—that the background, sensational or imaginal, is of fundamental importance to the meaning of perception. The kinaesthetic and organic sensations and the imaginal processes under our conditions have consistently formed a setting for the central visual complex, and have supplied more of the derived significance than the 'directly' initiated processes themselves. But we should not go so far as to contend that this 'context' is the meaning,[74] since the process itself is always to be considered as distinct from its function. When meaning is described no single process or group of processes, focal or marginal, can be looked upon as the meaning of the whole complex, but we must consider all of the integrated members as a unit. When we consider that meaning presents stages of elaboration, the lowest being the bare apprehension of a 'meaningless' object, the directly evoked process become of more importance to the significance in the lower stages than the 'fringe' or 'context.' This latter then functionates as a forced searching or seeking for derived, elaborate meaning, which usually fails. Nevertheless, as meaning becomes more elaborate, the accessory processes acquire increasing importance.

Since we have contended for an intimate relation between meaning and process, we cannot agree with the exponents of 'imageless' thought that the two are separate processes. Moore,[75] who has attempted to prove by experiment the presupposition, seems to have fallen into a twofold error; first, he has failed to take into account the stages of elaboration of meaning and, secondly, he has assumed that, if imagery and meaning were synonymous, then imagery must refer to the stimulus itself. Only very elaborate, derived meanings are considered in his results, and as the introspections of our observers show that perceptions under such conditions are very complex, we suspect that Moore left out of account the greater share of contributory processes. As regards imagery, our own results make it appear that the memory-image of the object, whether visual or kinaesthetic, was

[73]James, W., *op. cit.,* I, 258; Bagley, W. C., The apperception of the spoken sentence; a study in the psychology of language, *Am. J. of Psychol.,* 1900, xii, 80; Wallaschek, R., *Psychologie und Pathologie der Vorstellung,* Leipzig, 1905, 188; Lehmann, A., *Grundzüge der Psychophysiologie,* Leipzig, 1912, 603; and Titchener, E. B., *A textbook of psychology,* N. Y., 1915, 367ff.

[74]Titchener, E. B., *ibid.,* 367.

[75]Moore, T. V., The temporal relations of meaning and imagery, *Psychol. Rev.,* 1915, xxii, 177.

much less frequent than images with derived meanings all of which bore a direct relation to the total signification. A comparison, then, of the reaction-times of meaning and of imagery, directly referring to stimulus, would not be a fair determination of the relation of the meaning to imagery, since a good number of the relevant imaginal processes have been disregarded. A qualified statement of this problem by Tolman,[76] while it specifies too far, accords better with our own results.

Our experimental researches seem, then, to justify the following generalizations:

1. 'Visual' perceptions present a number of typical formations, each of which presents its own peculiar mode of integration and is marked by a distinctive temporal course. These formations are variously based upon three kinds of mental processes, (1) visual sensations directly evoked by stimulus, (2) kinaesthetic and organic sensations indirectly evoked (peripheral accessories), and (3) imaginal materials drawn from various sources (central accessories).

2. All 'visual' perceptions of our class have the same general constitution. The visual sensations, directly evoked by stimulus, are usually the clearest processes within the perceptual pattern, while the accessory processes lie chiefly in the region of moderate and limited clearness. The obscurest processes were, for the greater part, not relevant to the perception. During the temporal course of the perception, typical modifications occur, *i. e.,* (1) there is a gradual decrease in the number and clearness of the processes from phase to phase, and (2) there is a readjustment of the various factors themselves, the visual sensations tending toward obscurity while the accessory processes become relatively more prominent.

3. Spatial perceptions of the kind exemplified in our experiments fall into four classes as regards the kind of functions which they perform; namely, figurational ('nonsense' figures), depictive (presentations of specific objects), abstract (reference to a kind or class of objects), and symbolic (a secondary meaning which transcends the 'presented' object). The first correspond to objects which are frequently called 'meaningless,' the second to the customary apprehensions of sense (perception taken in the usual way), while the third and fourth suggest functions which are either intermediate between or else common to perception and thought.[77]

4. The component processes fulfil different functions

[76]Tolman, E. C., More concerning the temporal relations of meaning and imagery, *Psychol. Rev.,* 1917, xxiv, 138.

[77]A special study of these 'higher' forms of perception is well advanced in the Laboratory.

according to the degree of cognitive elaboration. Where the meaning is minimal or slight (as in the bare apprehension of an irregular 'nonsense' figure), the processes first bear the meaning and secondarily reenforce (in the form of effort or intent) a self-instruction to seek further significance in the figure. Where the meaning is moderate or complex, the accessory processes may give in addition a value to the object perceived (appreciative function) or they may establish it in its spatial and temporal relations (localizing function).

5. Meaning is only loosely correlated with the number and degrees of clearness of the indirectly initiated processes (peripheral and central accessories).

6. Kinaesthetic processes are not invariable components of spatial perceptions.

EXTATIC INTOXICATION IN RELIGION*

By James H. Leuba, Bryn Mawr College, Penna.

In the religions of nearly all savage and semi-civilized peoples, extatic intoxication is regarded as communion or union with the divine; and even in the higher religions, similar conditions enjoy the same consideration. Why should intoxication and extasy be assigned the supreme place in religion?

Religious intoxication is not always produced by drugs. It may arise from physical excitement, from dancing, for instance; or it may be generated by psychic means; those used by the Christian mystics. Although in this brief paper I shall be able to do little more than indicate the connection that exists between drug intoxication and the higher mysticism, I hope to show that despite the diversity of their origin they are closely related to one another by certain psychological characteristics, by the purpose they serve, and by the significance usually ascribed to them. I shall take up successively the several classes of means used to produce intoxication in religious ceremonies, the usual explanation of the exalted character ascribed to these states, and the deeper, more fundamental reason for this fact.

I had probably better say at the outset that I have no intention of reducing mysticism to drunkenness. For the best of the Christian mystics I have a real admiration; they are noble men who, on the whole, have deserved well of humanity. It seems to me childish, however, to pretend to dispose of the subject, as religious writers are wont to do, with vague high-sounding words such as ineffable, infinite, unutterable, absolute, divine. That is not the way to make intelligible the nature and the function of mysticism; it merely encourages a romantic megalomania already too conspicuous among religious believers.

Chemical means, i. e., drugs, are employed almost exclusively by uncivilized peoples in order to produce intoxication during religious ceremonies. Brinton tells us that "in every savage tribe we find a knowledge of narcotic plants which were employed to induce strange and vivid hallucinations or dreams . . . . .The negroes of the Niger had their 'fetish

* The substance of a paper read before the American Psychological Association, December, 1913.

water', the Creek Indians of Florida their 'black drink' for this purpose. In many parts of the United States the natives smoked stramonium, the Mexican tribes swallowed the *peyotl* and the snake-plant, the tribes of California and the Samoyeds of Siberia had found a poisonous toadstool; all to bring about communication with the Divine and to induce extatic visions."[1] The Indians of New Mexico who are "unacquainted with intoxicating liquors . . . find drunkenness, in the fumes of a certain herb smoked through a stone tube and used chiefly during their religious festivals." Among the old Mexicans, a seed called *Ololiuhgue* entered into a vision-producing "divine medicine," which could be obtained only from the priests.[2]

"In the Indic and Iranian cult there was," we are told, "a direct worship of deified liquor analogous to Dionysiac rites." It has even been maintained that the whole Rig Veda is but a collection of hymns for soma worship. The drinking ceremony was accompanied by magical incantations and by religious invocations. During the frequent libations that marked the sacrifice of soma, the officiating priest asked repeatedly for inspiration. He offered the liquor with these words: "O, Indra, accept our offering . . . drink of the soma, thou the friend of prayer and of the liquor; well disposed God, drink in order to intoxicate thyself." "I pour it out into the double cavity of thy belly; may it spread through thy members; may it be sweet to thy taste; may it steal upon thee, O deliverer, veiled as women seeking a *rendez-vous*. Hero with the strong neck, full bellied, strong of arms, O Indra, praised by many, accept the pressed out soma, father of divine energy."[3]

Modern India has not renounced the use of drugs in religious ceremonies. The India Hemp Commission appointed by the English Government to investigate the use of hemp drugs in its Hindoo possessions, reported that several hemp preparations are "extensively used in the exercise of religious practices." They found evidence of the "almost universal use of hemp drugs by fakirs, jogis, sanyasis, and ascetics of all classes, and more particularly by those devoted to the worship of Siva."[4] The hemp plant is believed by priests and people to be a special attribute of that god.

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[1] David Brinton, *The Religions of Primitive Peoples*, pp. 67.
[2] H. H. Bancroft, *Native Races*, vol. I, pp. 566-567.
[3] Galand and Henry, *L'Agnistonia*, vol. I, pp. 162, 155, 249; vol. II, p. 311.
[4] *Report of the India Hemp Commission*, 1893-94, Vol. I, pp. 160, 161, 165.

Wine drunkenness was conspicuous in the worship of Dionysos. To the effect of the wine was added that of dancing, music, shouting, and expectation of divine extasy.[5]

But drugs are not the only means of securing the blessed intoxication in which people of every degree of culture find delight. Rhythmic bodily movements, sufficiently violent and long continued, yield results similar in several respects to those of alcohol, stramonium, cohoba, and other drugs. There are in Grosse's *The Beginnings of Art,* interesting descriptions of Australian dances ending in a condition of extatic trance.[6]

As soon as a somewhat spiritual conception of divinity arose, drugs and mechanical means could no longer be regarded as proper means of approaching it. These grossly material methods appeared incongruous with the god-ideas that were taking shape. Furthermore, the disagreeable after-effects of these practices were not easily reconcilable with the theory of god-possession. Yet intoxication was too delightful and gratified too many deep needs to be given up. Thus arose the problem of finding a method of intoxication consistent with the higher conception of the divine nature. This problem was solved by the discovery of psychic methods, which, associated with drugs, appear already in Dionysiac and in Soma worship. In the Yoga practices, physical and psychical means are equally employed. In Christian mysticism, only the latter are acknowledged, although physical influences have not ceased to lend their aid. Even at the present day, physical means are not altogether excluded; in so-called "revival meetings," for instance, the monotonous repetition of rhythmical songs, accentuated by shouts and bodily movements, help to produce a condition similar to that in which the dervish attains partial anaesthesia and visions of Allah.

Why this extraordinary association of extatic intoxication with the gods? The common and well-known explanation is that intoxication in bringing visions and, with them, alleged superhuman powers of healing, of making rain, of destroying enemies, of forecasting the future, of controlling spirits, etc., raises man to the level of the gods.

Revelations and specific powers account perhaps sufficiently for the connection established in the unenlightened mind between intoxication and a higher world; but these characteristics do not fully explain its irrestible attraction. There are other

[5] See the description in Erwin Rohde's *Psyche, Seelencult und Unsterblichkeitsglaube,* 4th ed., Tubingen, 1907, vol. II, pp. 9-10.

[6] See chap. III.

and more fundamental reasons than those for the supreme place granted in religion to intoxication and extasy. The allurement of intoxication arises not so much from the belief that it affords esoteric knowledge and a share in the power of the Invisibles, as from the gratification it provides for certain deep needs and cravings. The truth of this statement is borne out by the fact that when intoxication ceases to be regarded as union with God and as a source of superhuman power, it continues to inspire the pen of the poet and to entice the unwary often beyond his power of resistance.

Some of these more fundamental values are indicated in the quotations I have given from religious customs of various peoples. They will appear more clearly and completely in instances of intoxication disconnected from any relation with religion. I quote one of the classical descriptions of the wonders worked by alcohol, "I send you," writes Colonel Ingersoll, "some of the most wonderful whiskey that ever drove the skeleton from the feast or painted landscapes in the brain of man. It is the mingled souls of wheat and corn. In it you will find the sunshine and the shadow that chased each other over the billowy fields; the breath of June, the carol of the lark, the dew of night, the wealth of summer and autumn's rich content, all golden with imprisoned light. Drink it, and you will hear the voices of men and maidens singing the 'Harvest Home' mingled with the laughter of children. Drink it, and you will feel within your blood the star-led dawns, the dreamy, tawney dusks of many perfect days. For forty years this liquid joy has been within the happy staves of oak longing to touch the lips of men." If all this and nothing worse was alcohol's gift to man, it would be in truth a "divine" beverage.

The information that has resulted from careful observation of the effect of narcotic drugs is still far from complete. We know, however, that their action may vary from person to person, and even in the same person under different circumstances, I shall mention only the more usual and constant effects to which these drugs owe their place in religion as well as outside of it. The drug devotees disregard all except those particular effects which are to them desirable; my purpose authorizes me to do likewise.

The effect of mescal (anhalonium lewinii), the very remarkable drug used in the religious festivals of Mexican and American Indians, has been studied by Dr. Weir Mitchell, Havelock Ellis, and others. The most noteworthy of its effects are marvellously beautiful color-hallucinations. Of these

the first investigator named wrote, "The display which for an enchanted two hours followed, was such as I find it hopeless to describe in language which shall convey to others the beauty and splendor of what I saw."[7]

Although there is a certain insensitiveness to fatigue, motor weakness and disinclination to activity is experienced. Ellis who notes this fact, remarks that this only throws "the subject of mescal intoxication more absolutely at the mercy of the waves of unfamiliar sensory impetus which strike him from every side. Every sense is affected . . . the simplest food seems to possess an added relish . . . and to the sense of touch, the body seems as unfamiliar as everything else has become." "The 'trailing clouds of glory,' the tendency to invest the very simplest things with an atmosphere of beauty, a 'light that never was on sea or land,' the new vision of even 'the simplest flower that blows,' all the special traits of Wordsworth's poetic vision correspond as exactly as possible to the actual and effortless experience of the subject of mescal."[8]

A uniform physiological effect of the drugs with which we are concerned is a reduction of the efficiency of the higher nervous system. They produce a partial break down of the interrelations by which the higher nervous centres exercise their control over the lower. The hilarious mood into which *cannabis indica,* ether, alcohol, and other drugs plunge their devotees, is due probably in part to this physiological action. In the description of his experiments with *cannabis indica* and ether, Dunbar wrote, "Time seemed to have no existence. I was continually taking out my watch thinking that hours must have passed, whereas only a few minutes had elapsed. This, I believe, was due to a complete loss of memory for recent events." Amnesia accounts, in part at least, for the freedom from all care and worry noted by this experimenter as well as by every other.

The beginning of the action of hasheesh is described thus by Dr. V. Robinson,[9] "The flood of laughter was loose, the deluge of mirth poured forth." One of the persons with whom he was experimenting exclaimed, "Cast aside all irrelevant hypotheses, and get to the laughing. I proclaim the supremacy of the laugh, laughter inextinguishable, laughter eternal, the divine laughter of the gods."

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[7] *The effect of Anhalonium Lewinii,* Brit. Med. Jr., vol. II, 1896, pp. 1625-1628.

[8] *Popular Science Monthly,* vol. LXI, 1902, pp. 52-71.

[9] *An Essay on Hasheesh,* Med. Rev. of Reviews, New York, 1912.

The more important psychological consequence of the reduced efficiency of the higher nervous centres is the partial or total removal of the checks imposed upon us by society and logic. This means a turning away from the insistent purposes of life, a relief from daily tasks and besetting unpleasant memories. The "primary" self enjoys a period of rest from the war waged against it by the social self. Thus, there comes recovery from the insufferable staleness to which we are sometimes brought by the unremitting demands of civilized life. Great and many are the evils into which people are enticed simply through the dullness of their existence. Tormented by the yearning for something to stir up the sluggish organism and restore a keen sense of life, man is often induced to seek excitement in drugs and in other forms of stimulation.

The need for relaxation by excitation is so universal that means of gratification have everywhere, and at all times, been sanctioned by society. Sacred and secular festivals, to which various purposes may be assigned, but which serve primarily to refresh through relaxation, form a part of the order of every society. When the faculty of Paris threatened to abolish the Feast of Fools, a petition was presented with the following argument, "Wine casks would burst if we failed sometimes to remove the bung and let in air. Now we are all ill-bound casks and barrels which would let out the wine of wisdom if by constant devotion and the fear of God we allowed it to ferment . . . .Thus on some days we give ourselves up to sport, so that with the greater zeal we may afterwards return to the worship of God." When one means of refreshing the organism becomes impossible another is substituted.

The removal of social checks and of logical constraint manifests itself in a delightful sense of freedom and of power. In intoxication the galling limitations of our ordinary selves seem gone. One of Robinson's subjects exclaimed, throwing off his blanket, "Throw off the bonds of all existence." To feel that impediments vanish before the fiat of one's will, that one is equal to every demand and can soar above all obstructions, is an entrancing experience indeed. What matters it, if, as in ordinary dream, this conviction be unfounded? The delight remains a real part of the experience.

The weakening of critical ability leads, in addition, to the liberation of the fancy. In intoxication one enjoys all the pleasures of untrammeled imagination. Its quality, judged objectively, may not be high, but the subject thinks otherwise and is proudly happy.

Perhaps the most insidious of the allurements of ecstatic intoxication is an intensification of the indescribable sensational and affective background of consciousness. If normally the vital organs are only faintly represented in consciousness, they provide nevertheless an essential part of the background of consciousness. The significance of these obscure feelings is well known to the student of mental disorders; their disappearance or alteration may cause profound mental perturbation and may lead to strange hallucinations. The first stages of intoxication instead of removing, apparently intensify both the sensational and the pleasurable aspects of this somatic background. When the brakes and the fly wheels which control the primary self are removed, it seems as if vital organs reawakened to sentiency, and their multitudinous voices are lifted in a paeon of life. Nothing but sexual erethism compares with the delights of this awakening of certain ordinarily silent parts of the organism.

Philosophic poets may interpret this experience as an upward surge of the Breath of Life, the *Elan Vital,* freed from the opposition of that Other, the alien Reason. It may please them to think that "through the intermediacy of organic life, we correspond, if confusedly, with the universe." Or, they may turn to the speculations of Frederick Meyers, and see in it a transient reinvasion of the focus of consciousness by vital parts of the organism which, in the far distant past, were the chief source of sentiency. Be that as it may, we shall, I trust, agree that the primary and essential value of the intoxication consciousness to the performer of religious rites, lies not in any alleged superhuman knowledge and power, but in other very substantial results. Intoxication and extatic states possess in themselves,—i. e., independently of an interpretative connection with the divine—a delightful, sensuous, rapturous quality; they bring deliverance from the fatigues, the restraints and tensions of the daily struggle, and they create a sense of unlimited possibility and exhaustless energy. In these effects, characteristic alike of the extasy of the Christian mystic and of the drug intoxication of the lowest savage, is to be found the deeper significance of the notion that in extasy man communes with the gods.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

Communicated by E. B. Titchener and E. G. Boring

XXXVII. The Weber-Fechner Law and Sanford's Weight Experiment

By Myrl Cowdrick

In the Cornell Laboratory we have records ranging from 1906 to 1917 of 89 performances of Sanford's weight experiment[1] by 48 observers. Ten of these observers performed the experiment only once; 32 of them did it twice; four, three times; and one, four times. Thus of the 89 experiments[2] 43 were performed after an initial experiment, *i. e.,* after a certain amount of practice. The average weight (in grams) of every one of the five piles of envelopes was computed both for the total 89 experiments and for the 43 experiments performed after some practice. Since the five piles in this experiment mark off four equal sense-distances, we may arbitrarily call the intensities of sensation (S) for the successive piles, 0, 1, 2, 3, and 4. The corresponding averages of the stimulus (R, av. weight of the envelopes in a pile in grams) are then as follows:

S:	0	1	2	3	4
R (av. total 89 exps.):	7.80	16.97	31.71	53.53	84.11
R (av. 43 exps. after practice):	7.75	16.53	30.43	52.17	83.93

The ratios between these two sets of R's are respectively:

2.17, 1.92, 1.69, 1.57;
and 2.14, 1.84, 1.71, 1.61.

Since the ratios are in neither case constant the Weber-Fechner Law is not realized, although it is approximated. The continuous decrease of the ratio from the lighter to the heavier piles shows that the actual curve is one of less eccentricity than is the logarithmic curve of the Weber-Fechner formula.[3] One such less eccentric function is that

[1] E. B. Titchener, *Experimental Psychology,* vol. II, 1905, pt. i, 33f.; pt. ii, 82ff.; E. C. Sanford, *A Course in Experimental Psychology,* 1898, 340f., 413f.

[2] The cases preceding add up to 90, but the data for the first trial by one observer who did the experiment twice are missing.

[3] Of course we must not generalize for a range wider than the actual limits of our experiment. The decreasing ratios might increase with still heavier piles, as do Ebbinghaus' ratios for brightness: 2.3, 2.1, 2.1, 1.8, 1.7, 1.7, 2.0; H. Ebbinghaus, *Grundzüge der Psychologie,* I, 1905, 520.

proposed by Fullerton and Cattell,[4] in which S is proportional to the square root of R.[5] The general equations for these two functions are:[6]

Weber-Fechner $S = k \log R + a$
Fullerton and Cattell: $S = c\sqrt{R} + b$

We adjusted both sets of averages (the 89 cases and the 43 cases after practice) by the method of least squares[7] to these two equations, thus determining the most probable values of the constants $k, c, a,$ and b.

Since the ratios between the two lightest piles (2.17 and 2.14) depart furthest from constancy, it appears that the Weber-Fechner Law holds more closely for the four heavier piles. Therefore we repeated our computations, omitting the average for the lightest pile.

The most probable equations resulting from all eight adjustments by the method of least squares are given in Table I.

TABLE I

MOST PROBABLE EQUATIONS BY THE METHOD OF LEAST SQUARES

	Weber-Fechner $S = k \log R + a$	Fullerton and Cattell $S = c\sqrt{R} + b$
Average of total 89 experiments:		
5 intensities ($S = 0$ to 4)......	$S = 3.89 \log R - 3.64$	$S = .623\sqrt{R} - 1.616$
4 intensities ($S = 1$ to 4)......	$S = 4.40 \log R - 4.52$	$S = .592\sqrt{R} - 1.383$

[4] G. S. Fullerton and J. McK. Cattell, *On the Perception of Small Differences,* 1892, esp. 152ff.

[5] The formula of Fullerton and Cattell is simply a special case of the relation proposed by Plateau, Grotenfelt, and others, *viz.,* that equal R-ratios correspond to equal S-ratios. The formula for this hypothesis is $S = c R^p + b$. In the formula of Fullerton and Cattell, $p = 0.5$. See Titchener, *op. cit.,* pt. ii, 69; W. Wundt, *Physiologische Psychologie,* I, 1908, 640.

[6] The constants of proportionality are k and c. The constants a and b are so taken that the values of S come out in the arbitrarily assumed scale: 0, 1, 2, 3, 4. Obviously the value $S = 0$ is a fairly heavy weight, but the constants could readily be altered without readjusting the equations, for any other scale of S that might be desired. The constant a also includes the unknown value "$k \log r$," since the unit of R is taken here as the gram and not as the value of the stimulus limen, r; see Titchener, *op. cit.,* pt. ii, xxviii.

[7] We used a linear method of adjustment by least squares; see L. D. Weld, *Theory of Errors and Least Squares,* 1916, 73f., 89f., and, in general, 65-103.

An adjustment to Plateau's ratio-hypothesis (see note above) would necessarily have given a closer approximation, since there are three constants instead of two to be determined; but to be able to adjust with little error for three constants when there are only four or five observational values would not be positive evidence in favor of the hypothesis. Since the adjustment of an exponential equation is a laborious matter (*cf.* Weld, *op. cit.,* 178-180) and since the results would have little significance, we have not attempted it.

TABLE I—*Continued*

Average of 43 experiments after practice:		
5 intensities ($S = 0$ to 4)......	$S = 3.46 \log R - 3.01$	$S = .677 \sqrt{R} - 1.892$
4 intensities ($S = 1$ to 4)......	$S = 4.45 \log R - 4.56$	$S = .587 \sqrt{R} - 1.311$

We can measure the adequacy of the two hypotheses in all cases if we compute from every equation the theoretical values of R which that equation gives for every value of S, subtract this theoretical R from the R observed for the same S, square this difference, and then find the sum of these squares for all values of S. This sum of the squares of these deviations of observed values from theoretical values constitutes a measure of the degree with which an hypothesis is approximated. In the instances with which we are at present dealing the sums will, however, tend to be higher when the five squares for the five intensities are added than when the four squares for the four intensities are taken. Accordingly we have divided every sum by the number by which the normal equations are overdetermined (three or two)[7a], in order to get comparable values. These measures are given in Table II.

TABLE II

MEASURES OF THE DEGREE WITH WHICH THE TWO HYPOTHESES ARE APPROXIMATED BY THE OBSERVED DATA

Sums of the squares of the deviations of the average observed values from the theoretical values (computed from the least square equations of Table I), divided by the number of overdeterminations.

	Weber-Fechner	Fullerton and Cattell
Average of total 89 experiments:		
5 intensities............	28.014	5.145
4 intensities............	6.783	2.578
Average of 43 experiments after practice:		
5 intensities............	20.180	25.527
4 intensities............	3.562	5.223

From inspection of Table II we may draw for our data the following conclusions:

(1) In the complete experiment (all 89 cases, 5 intensities) the formula of Fullerton and Cattell represents the actual results much more adequately than does the Weber-Fechner Law (5.145 *vs.* 28.014).

(2) The limitation of the range of the experiment by the omission of the least intensity increases the approximation of the observed data

[7a] *Cf.* W. W. Johnson, *Theory of Errors and Method of Least Squares,* 1915, 108.

to both formulae, but still leaves a decided difference in favor of the formula of Fullerton and Cattell (2.578 *vs.* 6.783).

(3) With practice the observed results approach the form of the Weber-Fechner Law and depart from the function of Fullerton and Cattell, so that the former hypothesis now becomes the more representative (25.527 *vs.* 20.180).[8]

(4) With the more limited range of intensities after practice the approximation to both hypotheses is greatly improved, as it was without practice; and the Weber-Fechner Law remains the more representative hypothesis, as it was after practice for the full range.

(5) If the equations of Table I are plotted together with the curves of the observed values, it will be found that the curve of actual values lies always between the two hypotheses; with practice, nearer the logarithmic curve; without practice, nearer the curve of the square root of the magnitude. Thus it appears that the formula of Fullerton and Cattell deviates from the logarithmic function of the Weber-Fechner Law in the proper direction, but deviates too far. Whether after great practice with Sanford's weights any correction of the Weber-Fechner formula in the direction of the equation of Fullerton and Cattell would be necessary, cannot be said.[9]

XXXVIII. An Example of the Fractionation of Data From the Method of Constant Stimuli for the Two-Point Limen

By L. B. Hoisington

We present herewith the results of a determination of the limen of dual impression upon the skin, by the method of constant stimuli,[1] in which the data have been fractionated and dealt with separately for every fraction. Our problem is analogous to that undertaken by Fernberger for lifted weights (*The Effects of Practice in Its Initial Stages in Lifted Weight Experiments and Its Bearing upon Anthropometric Measurements*).[2] We have, however, fractionated more completely than Fernberger, and have thus been able to extend our treatment to more general conclusions.[3] On the other hand, we have intended our case to be merely an illustration of the manner in which such data may be treated, for we used but two observers; whereas Fernberger had ten subjects.

The experimental work was performed by A. M. Palmer and P. R. Dickinson, both of whom accepted opportunities to enter the National Service at a time when the computation of their results was

[8]Titchener notes that the Weber-Fechner law is approached with practice; *op. cit.*, pt. ii, 83.

[9]We have noted that Plateau's law will approximate the observations, but that with so few determinations this approximation indicates little. Merkel's law (*cf.* Titchener, *op. cit.*, pt. ii, 69; Wundt, *op. cit.* I, 635 ff.), the straight line, is obviously inappropriate.

[1] E. B. Titchener, *Experimental Psychology*, II, 1905, i, 92-104; ii, 248-258; E. G. Boring, this Journal, 28, 1917, 280-293.

[2]S. W. Fernberger, this Journal, 27, 1916, 261-272.

[3] See Boring, this Journal, 27, 1916, 315-319; Fernberger, *Psychol. Bull.*, 14, 1917, 110-113; Boring, this Journal, 28, 1917, 280-293.

hardly more than begun. The writer is, therefore, indebted to these experimenters for data discussed in the present paper.

P and *D* worked together, the one acting as observer when the other experimented. They worked upon the forehead with the Griesbach aesthesiometer, using blunted conical rubber points applied at a pressure of 10g. The observer lay upon a couch; after every fifth series he rose and moved about in order that he should not become sleepy. He made his judgments before the points were removed from the skin. His ability to make the kind of judgment required was assured by preliminary practice-series. The experiment proper was concluded in 15 sessions; 10 or 15 series were taken at every session. A session always began with 5 'warming-up' series, which were discarded. The instructions were as follow: "After the signal 'ready,' you will be given a cutaneous stimulus on the forehead. You are to make an immediate judgment of 'one' or 'two'. The judgment 'two' is to stand for two discrete impressions; all other impressions are to be judged as 'one.' Make every judgment independently, without reference to any preceding impression. Keep as constant an attitude as possible;[4] if for any reason your attitude changes, so that you are in doubt about a particular impression, ask to have it repeated."

Five values of stimulus were used: 10, 11, 12, 13, and 14 mm. The total number of series was 200. These, for purposes of fractionation, were divided into groups of 100, of 50, and of 25. For every one of these fractions (15 in all[5]) we have computed the limen and the measure of precision (h), using Urban's tables[6], the probable error of the limen[7], and the sum of the squares of the deviations of the actual percentages from the theoretical.

1. *Practice.* Table I shows the values of the limen for all fractions, and Table II the corresponding values of h. There is (in the regular sense of the term) no practice-effect. The limens for both *O*'s, on the contrary, increase slightly from the first 100 to the second 100. If we examine the 50's and the 25's we see that this increase is not regular. There may, nevertheless, be some general tendency toward increase which is partially obscured by the great variability that occurs when the number of cases is few. If we plot the curves for these limens, by groups of 25 series, we note a general tendency toward

[4] On the advantage of a constant attitude in psychophysical judgments, see S. S. George, this JOURNAL, 28, 1917, 1-37, esp. 33-36.

[5] *I. e.,* one group of 200 series, two 100's, four 50's, and eight 25's.

[6] In the case of the total group of 200 series there were six instances of half percentages. For these percentages we interpolated in Urban's tables for P and γP by fitting a parabola through three adjacent points. The method is more accurate than a straight-line interpolation. It makes no difference in the case of the half-percentages whether the interpolated point lies between the first two or the last two of the three points. On a similar case of parabolic interpolation see F. M. Urban, *The Application of Statistical Methods to the Problems of Psychophysics,* 1908, 124 f.

We checked the additions by the use of a checking table that gives the sums of the five values of Urban's tables for every value of x and p. This table is now in process of compilation by Dr. G. J. Rich; it is to be hoped that he may soon be able to publish it.

[7] For the manner of computation of the probable error of a limen, account being taken of the weighted number of cases, see Boring, this JOURNAL, 27, 317.

TABLE I

LIMENS (CM.)

Obs. *D*.

	Grouping by Series			
	By 25's	By 50's	By 100's	Total 200
1	12.119	12.348	12.214	12.348
2	12.551			
3	12.234	12.109		
4	11.981			
5	12.822	12.501	12.485	
6	12.229			
7	12.395	12.483		
8	12.548			

Obs. *P*.

	Grouping by Series			
	By 25's	By 50's	By 100's	Total 200
1	11.648	11.538	11.537	11.642
2	11.434			
3	11.452	11.532		
4	11.599			
5	11.658	11.578	11.667	
6	11.498			
7	11.710	11.783		
8	11.826			

TABLE II

MEASUREMENT OF PRECISION (*h*)

Obs. *D*.

	Grouping by Series			
	By 25's	By 50's	By 100's	Total 200
1	.4052	.3994	.4498	.4638
2	.3987			
3	.5740	.4986		
4	.4391			
5	.3565	.4151	.4828	
6	.4857			
7	.5153	.5696		
8	.6136			

Obs. *F*.

	Grouping by Series			
	By 25's	By 50's	By 100's	Total 200
1	.4128	.3837	.3970	.4540
2	.3719			
3	.3368	.4013		
4	.4765			
5	.3966	.4185	.3620	
6	.4424			
7	.2420	.3223		
8	.3834			

increase which appears by inspection to be almost as marked as in the true practice-curve of *h* for Fernberger's Subject I.[8] If, finally, we compute by the method of least squares the most probable straight line for our two curves, we find a general tendency throughout the span of 200 series for the limen to increase. The increase thus indicated is for *D* 0.292 mm. (2.4%), and for *P* 0.267 mm (2.3%).[9]

The practice-effect upon *h* is not the same for the two *O*'s when all eight of the groups of 25 are considered. Plotting the curves by series, as above, we find for *D* the expected increase in the measure of precision to be 0.161 (35%), while the corresponding value for *P* is —0.059 (13%), a slight decrease. If, however, we plot the curve for the first six of the 25's there is, as in the case of *D*, a noticeable increase; the amount is 0.086 (19%). This discrepancy is accounted for by the fact that the last sessions were held after *P*, together with several others of his college fraternity, had enlisted in the army. It is probable that the constancy of his attitude was thereby greatly disturbed.

2. *Dependence of a Limen upon the Number of Observations on which It is Based.* Table III shows the probable errors of the limens. Since these values vary inversely with the square root of the weighted number of cases, we get a decrease of the average probable error as the number of series is increased from 25 to 200. An eight-fold increase in number of series (25 to 200) reduces the probable error of the limen, in the case of *D*, to one-third (.1104 to .0378) and, in the case of *P*, to less than one-third (.1294 to .0378).

The question how many series we must take in determining a limen depends in part upon what we mean to do with the limen when we get it. If we intend, as is usual, to compare it with another limen, then we must take enough series to reduce its probable error to a value which will give to the difference between it and the limen with which we compare it the probable correctness that we desire. We can not settle mathematically what probable correctness we shall regard as significant; we must decide the question in accordance with our scientific judgment in the particular case. Probably most psychologists would at present regard a probable correctness of 98% as very significant and one of 75% as not very significant.[10]

Suppose, for example, that we should wish to compare a limen taken from *D* with another limen which shows on the average the same degree of precision. If we found a difference of .484 mm. between the limens, then we can show by the proper computations that 25 series would be enough to establish this difference with a probable correctness of 98%. If the difference were only .165 mm., 200 series must needs have been taken to give a probable correctness of 98%. But 25 series would give for a difference of about .165 (exactly .156 mm.) a probable correctness of 75%. It appears, therefore, under our assumed condition, that 200 series would give a probable correctness of about 98% where 25 series would give a probable correctness of about 75%. It also appears that highly significant differences could be obtained from less than the usual 100 series if the difference were at all large.

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[8] Fernberger, *Psychol. Rev. Monog.*, No. 61, 1913, 42.

[9] For a general discussion of the effect of practice upon the limen for dual impression, see G. M. Whipple, *Manual of Mental and Physical Tests*, I, 1914, 254 f. and Bibliography; esp. L. Solomons, *Psychol. Rev.*, 4, 1897, 246-250; G. Tawney, *Philos. Stud.*, 13, 1897, 163-222.

[10] This JOURNAL, 28, 1917, 459.

TABLE III

PROBABLE ERRORS OF THE LIMEN

Obs. *D.*

	Grouping by Series			
	By 25's	By 50's	By 100's	Total 200
1	.1187	.0862	.0539	.0378
2	.1262			
3	.0932	.0719		
4	.1116			
5	.1352	.0830	.0532	
6	.1036			
7	.1001	.0658		
8	.0947			
Average	.1104	.0742	.0535	.0378

Obs. *P.*

	Grouping by Series			
	By 25's	By 50's	By 100's	Total 200
1	.1168	.0882	.0604	.0369
2	.1283			
3	.1395	.0848		
4	.1057			
5	.1220	.0826	.0709	
6	.1123			
7	.1869	.1006		
8	.1244			
Average	.1294	.0891	.0656	.0369

Suppose again, to make the illustration more concrete, that we consider the probable correctness of an individual difference between D and P. In Table IV we have computed[11] the probable correctness of

TABLE IV

PROBABILITY OF DIFFERENCE

	Probable Correctness of Difference					
	Average Difference		Maximal Difference		Minimal Difference	
Series	$\frac{D}{P.E._D}$	P. C.	$\frac{D}{P.E._D}$	P. C.	$\frac{D}{P.E._D}$	P. C.
200	13.55	100.00				
100	8.84	100 00	11.77	100.00	6.14	100.00
50	6.27	100.00	8.17	100.00	2.64	96.25
25	4.45	99.75	7.45	100.00	.93	73.36

the difference between the limens of D and P when the entire 200 series are taken, and when the minimal, maximal, and average differences are taken for the groups of 100 series, 50 series, and 25 series. We find that the probable correctness is 100.00% ("mathematical certainty") for the difference when 200 series are taken, and for any difference, even the minimal, between limens based on 100 series. If but 50 series are taken, we have 100.00% probable correctness for the difference between the averages of every set of four limens in the case of the minimal difference (between D's 2nd 50, and P's 4th 50), however, the probable correctness is 96.25%: a highly significant value, although not "mathematical certainty." Twenty-five series give 100.00% for the maximal differences, 99.86% for the averages, and only 73.36% for the minimal difference (between D's 4th 25 and P's 8th 25). If we regard 96% probable correctness as sufficiently significant (as most investigators would), we are justified in expecting that 50 series would be sufficient to establish the fact of an individual difference between D and P. Twenty-five series would usually give a difference of high probable correctness, yet not always, for the minimal difference is but 73%. The application to Fernberger's problem[12] is obvious; our individual difference is established by fifty series, not because practice-effect is considerably diminished, but because the number of observations attests the reliability of the indicated difference.

3. *The Approximation of the Actual Data to the Phi-Gamma Hypothesis.* Table V shows the sums of the squares of the deviations of the actual percentages from the theoretical percentages as determined under the Φ (γ)-hypothesis. The numbers, therefore, consti-

[11] The manner of computation of the values of Table IV as well as of the immediately preceding theoretical values is obvious from Boring, this JOURNAL, 27, 315-319.

[12] This JOURNAL, 27, 1916, 261 ff.

tute measures of the degree in which this hypothesis is realized in practice. The averages, for *D*, show that doubling the number of series tends approximately to halve the departure of the empirical data from the hypothesis. The same tendency is observable in the averages of *P*. The averages of the groups of 25, 50, and 100 series show a highly uniform ratio of decrease; here the averaging covers up the variations due to the unfavorable conditions already mentioned. It is clear, however, especially with *D*, that we approach the theoretical function as we increase the number of observations. This relationship suggests that the Φ (γ)-hypothesis is the correct hypothesis for these conditions, and that deviations from it are due in actual cases not to errors of theory but to errors of observation.[13]

The variability (M. V.) of these deviations also tends roughly to vary inversely with the number of the series. Two of the values for the groups of 25 are less than the value for the total group of 200 (*D*), although the average for the 25's is almost six times the value for the 200. For *P* the same relationship holds, although it is less extreme.

For *D* the Φ (γ)-hypothesis appears to be approached with practice. The departure of the second 100 from theory is only about one-eighth that of the first 100. Even when the errors of observation are increased by reduction of the number of the series to 50, the last 50 fit theory over five times more closely than the first 50. *P*'s results, however, show exactly the opposite relation.

TABLE V

SUMS OF THE SQUARES OF THE DEVIATIONS OF THE ACTUAL FROM THE THEORETICAL PERCENTAGES

Obs. *D*.

<table>
<tr><th rowspan="2"></th><th colspan="4">Grouping by Series</th></tr>
<tr><th>By 25's</th><th>By 50's</th><th>By 100's</th><th>Total 200</th></tr>
<tr><td>1</td><td>.012446</td><td rowspan="2">.025707</td><td rowspan="4">.007375</td><td rowspan="8">.002918</td></tr>
<tr><td>2</td><td>.070011</td></tr>
<tr><td>3</td><td>.001276</td><td rowspan="2">.002172</td></tr>
<tr><td>4</td><td>.004862</td></tr>
<tr><td>5</td><td>.020106</td><td rowspan="2">.006046</td><td rowspan="4">.000921</td></tr>
<tr><td>6</td><td>.002699</td></tr>
<tr><td>7</td><td>.015358</td><td rowspan="2">.000459</td></tr>
<tr><td>8</td><td>.009828</td></tr>
<tr><td>Average</td><td>.017073</td><td>.008596</td><td>.004148</td><td>.002918</td></tr>
<tr><td>M.V.</td><td>.013992</td><td>.008555</td><td>.003227</td><td></td></tr>
</table>

[13] Here we have a hint as to a method of separating errors of observation from errors of theory; *cf.* Urban, *Psychol. Rev.*, 17, 1910, 231 f.

Obs. *P*.

	Grouping by Series			
	By 25's	By 50's	By 100's	Total 200
1	.006310	.000199	.000537	.005683
2	.007635			
3	.024527	.003006		
4	.007560			
5	.025026	.019722	.005736	
6	.023411			
7	.030874	.005758		
8	.010099			
Average	.016934	.007173	.003142	.005683
M.V.	.009031	.006275	.002607	

With *D* practice is the more effective of these two factors. We approximate theory more closely in the second 100 than in the entire 200; practice more than makes up for the reduction of the number of series to one-half. The fourth 50 actually fit theory about seven times more closely than do the entire 200. If approximation to theory were, as it appears to be for *D*, a measure of the elimination of errors of observation and hence of the reliability of data, then we should do well to take many series, and to discard the early ones as merely furnishing practice.

Unlike *D*, however, *P* departs, as we have seen, farthest from the hypothesis in the last quarter of his work. This result may mean, on the face of it, that the hypothesis is not ideal for *P*; more probably however, certain disturbing factors increased in effectiveness during the course of the experiment.[14]

Finally, we may note that the absolute size of these measures of the deviation of practice from theory is comparable with the size of the measures upon which Urban assumes that the Φ (γ) is a better hypothesis for lifted weights than is the arctan-hypothesis.[15] One-third of our values (when divided by 3, the number of observations by which the limen is overdetermined) are less than the average of Urban's values (when divided by 5, the amount of overdetermination in Urban's case).[16]

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[14] The situation which finally resulted in P's departure from the university for military service appears to have unfitted him for observation; see above, p. 592.

[15] *Op. cit.*, 258.

[16] *Cf.* the formulae in W. W. Johnson, *Theory of Errors and Least Squares*, 1915, 108; Urban, *Arch. f. d. ges. Psychol.*, 16, 1909, 226.

XXXIX. The Effect of Absolute Brightness Upon Color Contrast

By Ruth L. Crane

Kirschmann[1] laid down the law that "simultaneous color-contrast is best when brightness-contrast is eliminated or reduced to a minimum."[2] This relationship, as Kirschmann pointed out, is implicit in Schmerler's earlier study.[3] Schmerler gives a table which shows that a "strong" contrast-effect occurs under conditions in which the brightness of the colored inducing field and of the grey field which undergoes induction are approximately equal; whereas the contrast-effect is "weak" when the one field is very much lighter or darker than the other.[4] Kirschmann tabulates the actual values of the brightnesses of gray disks (in degrees of Bk and W) which gave the "maximal value of contrast" with certain colored inducing fields.[5] His data are few, but the indication is that the relationship holds without regard to the saturation of the inducing field; fields of the same brightness but of different saturations induce maximal contrast upon fields of the same brightness, although the *amount* of the maximal effect does, naturally, depend on saturation. Neither Schmerler nor Kirschmann gives figures to show the manner in which the contrast-effect decreases when the contrasting field is made lighter or darker than the inducing field. Obviously, if we were to measure the contrast-effect of a constant inducing field, we could plot the amount of contrast-effect for varying brightnesses of a gray field, and could determine the nature of a function, in which the maximum would occur at the brightness of the inducing field. The form of this function constitutes a psychological problem.

Pretori and Sachs[6] measured the contrast-effect of a colored background upon a gray ring by determining how much of the color, complementary to the contrast-color, must be introduced into the ring in order just barely to neutralize the contrast-color. The results, on their face value, appear to contradict Kirschmann's law; hence we shall do well to examine them critically.

Both background and ring were made of rotating colored disks.[7] There were four experiments; in every one the background consisted of a color and black and white, and the ring of the same color (less of it, to kill the contrast) and black and white. The color-valence (C. V.) of a combination is measured directly by the number of degrees of color. The white-valence (W. V.) is measured by the number of degrees of white *plus* the white-equivalent of the black (in this case about 1.7% of the number of degrees of Bk) *plus* the white-equiv-

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[1]A. Kirschmann, *Philos. Stud.*, 6, 1891, 417-491.

[2]Pp. 475, 491.

[3]B. Schmerler, *ibid.*, I, 1883, 379-416; esp. 387ff.

[4]*Cf.* Schmerler, 388 (Table), and Kirschmann, 475.

[5]P. 476.

[6]H. Pretori and M. Sachs, *Pflüger's Arch.*, 60, 1895, 71-90. This paper is summarized, a little obscurely, by J. H. Parsons, *Introducton to the Study of Colour Vision*, 1915, 264-266. (A confusing misstatement occurs p. 265, l. 28.)

[7]*Cf.* also L. J. Martin, *Am. J. Psychol.*, 24, 1913, 33f., who does not, however, mention Pretori and Sachs.

alent of the color (about 25% in the case of red). The value of the W. V. is a direct measure of the brightness of the color. The ratio of the C. V. to the W. V. is a measure of the "saturation" of the color. (These relationships are Hering's.) The results may be summarized as follows.

Exp.	Background			Ring		
	C.V.	W.V.= Bright-ness	$\frac{\text{C.V.}}{\text{W.V.}}$ = Satura-tion	C.V.	W.V.= Bright-ness	$\frac{\text{C.V.}}{\text{W.V.}}$ = Satura-tion
I	constant	constant	constant	increased (proportionally)	increased	constant
II-1	increased	constant	increased	constant	decreased	increased (proport. to incr. in bckgr.)
II-2	constant	increased	decreased	constant	increased	decreased (proport. to decr. in bckgr.)
II-3	increased (proportionally)	increased	constant	constant	constant	constant

In Exp. I the background was kept constant. The C. V. of the ring was increased arbitrarily; then the W. V. was increased until the contrast-color of the ring was just eliminated. The necessary increase of W. V. was about proportional to the increase of C. V.; hence the saturation remained constant. In Exp. II 1, the C. V. of the background was increased arbitrarily and the W. V. was kept constant; hence the saturation of the background increased. The C. V. of the ring was kept constant and the W. V. decreased until the contrast-color was eliminated. The resulting increase in saturation of the ring was approximately proportional to the increase of saturation of the background. In Exp. II 2, the C. V. of the background was kept constant and the W. V. was increased; hence the saturation was decreased. The C. V. of the ring was kept constant and the W. V. increased until the contrast-effect was just about to appear. The decrease in saturation proved to be very nearly proportional to the decrease in saturation of the background. Finally, in Exp. II 3, the saturation of the background was kept constant by increasing the C. V. and W. V. in a constant proportion. Under these circumstances the contrast-effect in the ring was eliminated without any considerable change in the ring.

These results appear to show that the amount of color-contrast is dependent upon the saturation of the inducing field and is independent of the relative brightnesses of the fields. A contrast-effect produced

by a constant saturation is neutralized by a constant saturation, even though the brightness of the ring be increased while the background remains constant (I) or the brightness of the background be increased while the ring remains constant (II 3). The contrast-effect of an increasing saturation is neutralized by a saturation which continues in the same proportion to it, even though the ring be continuously darkened with respect to the background (II 1). Does Kirschmann's law break down under these conditions?

The generalizations of the table are, however, only approximate; and the actual deviations show a trend that is in accordance with Kirschmann's finding. In all of Pretori and Sachs' cases the ring is darker than the background, and generally very much darker. Thus, in Exp. I, when the ring is brightened, it approaches the background in tint and should tend to have more color induced in it; the actual saturation necessary for the elimination of the contrast-color should therefore increase gradually. If we examine the data (pp. 79f.), we find that the saturations of the ring do increase somewhat, just as Kirschmann's law would have led us to expect. The courses of the saturations of the ring in the two cases are:

0.7, 0.9, 1.2, 1.3, 1.3, 1.3, 1.3;
and 0.4, 0.7, 0.9, 1.1, 1.2, 1.3, 1.3, 1.4, 1.4.

In Exp. II 1, the ring begins as darker than the background and is made still darker; a constant saturation of background should continuously induce less color. Thus we should expect, under Kirschmann's law, that the actual increase in saturation of the background would require something less than a proportional increase in saturation of the ring. And we find our expectation justified in the averages of the two cases (pp. 82f.):

When the background changes as	1.00	:	1.25	:	1.50,
the ring changes as	1.00	:	1.07	:	1.17;
and when the background changes as	1.00	:	1.50	:	2.00,
the ring changes as	1.00	:	1.30	:	1.50.

In Exp. II 2 the background and ring grow lighter together; hence we might expect proportionality between the saturation of background and ring. The actual data (p. 85) show a much closer proportionality than occurs in any other case.

When background changes as	1.00	:	0.75	:	0.50,
the ring changes as	1.00	:	0.77	:	0.57.

Finally, with the increase of brightness-contrast due to the lightening background of Exp. II 3, we should expect the necessary saturation of the ring to decrease slightly. The actual average decreases (pp. 87f.) are as 1.00 : 0.95 : 0.92 and as 1.00 : 0.90 : 0.87 : 0.92.

Pretori and Sachs thus substantiate Kirschmann indirectly and even furnish a hint of the form of the function[8] when the induced field is darker than the inducing.

Ebbinghaus[9] notes that approximately equal brightnesses of the contrasting field are necessary for maximal contrast. He explains the decreased contrast-effect on grays lighter than the inducing color by the desaturating effect of light gray (*cf.* Hering's relation of W. V. to saturation, above); and the decreased effect on dark grays

[8]The deviations in Exp. I are consistent with the data that we shall present later.

[9]H. Ebbinghaus, *Grundzüge der Psychologie,* I, 1905, 239.

as due to irradiation. Since different factors are operative at the two extremes, one might expect the function to be asymmetrical.

Recently Cook and Kunkel[10] have published data in which "the law of greatest color contrast when brightness contrast is eliminated seems to hold . . . only to a certain extent. It is apparently partially counteracted by another tendency, namely, for the lighter rings to suffer greater contrast effect than the darker ones. . . . The two laws. . . . sometimes work together and sometimes cut across each other." The authors are far from dogmatic about this second law or tendency; they suggest further experiments, and phrase the problem: "Do lighter surfaces suffer more color contrast effect than darker ones, under equal conditions of brightness contrast between the contrast-inducing and contrast-suffering surfaces?"

It is not obvious upon what aspect of their data Cook and Kunkel base this suggestion. They may mean that a given color tends to induce more color upon a lighter gray, or else that a lighter color tends to induce more color upon a gray of the same tint. In the first case, this second tendency would operate to make the function that occurs under Kirschmann's law asymmetrical. When rings darker than their backgrounds are lightened the two tendencies would be additive; when lighter rings are made still lighter the tendencies would be in opposition; and whether the function rose or fell would depend on the dominant tendency. If the tendency of Kirschmann's law were the more effectual, then the function would simply slope off more gradually for the light rings than it rose for the dark; if the tendency for lighter rings to suffer more contrast-effect were dominant, then the function might continue to rise for the lighter rings, though less rapidly than for the darker. In the second case, we should find the function symmetrical with respect to the tint of the inducing ground, but greater amounts of induced color would occur for the lighter grounds.

Cook and Kunkel's data provide some evidence for the first conclusion. Of their 18 cases (p. 19), 5 show a continuous increase in the amount of induced color when the brightness of the ring is increased, 6 show an increase followed by a decrease, and 7 are equivocal. When all the results are averaged, the tendency is toward continuous increase. Every function is, however, based upon four points only, and the differences are small; hence, as the authors themselves point out, we must accept general conclusions with reserve.

The alternate conclusion, namely, that lighter colors induce, upon grays of the same brightness, more contrast-color, seems, at first glance, to be established. Hering Y, for example, is over six times as bright (by the flicker method) as Hering R and induces over five times as many degrees of BG. The correlation between the average amounts of induced color (3 O's) and the flicker-brightnesses of inducing backgrounds is about 79%. We could not presumably expect a higher value when the saturations are uncontrolled. Hering Y is probably of poorer saturation than Hering R and loses on that account.

Cook and Kunkel measured amount of contrast by the total degrees of color necessary on the second color-mixer to match the induced color of the ring. The amount of colored paper in a color-combination, however, by no means measures the amount of color *seen*. White paper has a much greater desaturating effect than black.

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[10]H. D. Cook and F. M. Kunkel, *Psychol. Rev. Monog.*, No. 96, 1916, 1-39; esp. 18f. 22, 39.

Hence 180°R + 180°Bk looks much redder than 180°R + 180° W, although the angular amount of R is the same. In Hering's terms—as we saw in our discussion of Pretori and Sachs—the amount of colored paper is one thing, the "color-valence;" the amount of color is another thing, the "saturation", and is the quotient of the C. V. by the W. V. (brightness). Cook and Kunkel deal with color-valences; Kirschmann was interested in the amount of color seen (*i. e.,* the saturation, if we grant the hypothesis that equal saturations, defined as above, appear equally colored). Cook and Kunkel do not give the data which permit the computation of the W. V.'s of their mixtures and hence of the saturations. The general indication of their data is that the increased contrast (C. V.) in both the cases we have discussed is roughly proportional to the increase in brightness (W. V.) upon which it depends; that is to say, the saturation (granting that Hering's relation holds approximately) does *not* tend to increase with lighter rings. In the first case, we may argue that the contrast-color (saturation) suffers as much from brightness-contrast in rings lighter than the inducing ground as it does in darker rings, but that more colored stimulus (C. V.) is required to produce a given amount of color (saturation) in the lighter rings. In the second case, we may urge that all colors of the same degree of saturation induce equal amounts of contrast-color in grays of the same brightness as the inducing color, but that lighter grays, which match the lighter colors in tint, require more C. V. in order that they shall appear equally colored.

Experimental

We have repeated Cook and Kunkel's experiment with various tints of a single color, with the intention of gaining more light on the proposed second law. The work was done in the Cornell Summer Session, 1917. There was time only for a single determination of every point; hence the work can be regarded as little more than preliminary.

We used three observers: Miss M. Cowdrick (*C*), Mr. H. D. Williams (*W*), and Dr. E. G. Boring (*B*).

We followed Titchener's general procedure[11], but made the ring by using three sizes of disks[12]. We secured constant illumination by working in a dark-room with artificial daylight[13]. A single "day-lite" unit (200-watt Mazda Type-C lamp) was placed 70 cm. in front of the color mixers and slightly above them. The observer was seated 2 m. from the mixers and looked under the lamp. He could not see the light directly. The size of the disks and the distance between the two motors duplicated the dimensions of Cook and Kunkel (p. 12). A large gray exposure-screen, which obscured both disks and background, controlled the time of exposure. The disks were never seen except when in rotation. In the early stages of making a match the observer was allowed long exposures. He was compelled, however, to make his final judgment of equality with a brief exposure of approximately 1 sec.[14]

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[11]E. B. Titchener, *Experimental Psychology,* I, i, 1901, 16, 19.

[12]Pretori and Sachs; Martin; *cf.* note 7 above.

[13]*Cf.* A. J. Brown Some Uses of Artificial Daylight in the Psychological Laboratory, *Am. J. of Psychol.,* 27, 1916, 427-429.

[14]A precaution which Cook and Kunkel did not observe, although they tried fixation. *V.* pp. 11, 20f.; also the fourth conclusion, p. 39. On adaptation, *cf.* Titchener, *op. cit.,* I, ii, 33.

Ideally we should have selected as inducing backgrounds three reds of equal chroma and very different tints. We lacked time, however, to carry through a psychological method for the equation of the chromas; and we feared to trust that equal saturations, according to the Hering formula, would give subjectively equal chromas. Instead we selected a standard R of middle tint (S); two much lighter R's, approximately equal in tint (actually of equal W. V.), one slightly better and the other slightly poorer in chroma than S; and two darker R's of equal tint, one slightly better and the other slightly poorer than S. Later we added another pair of still lighter R's and another pair of still darker R's. We checked the subjective equality of tint and the just noticeable subjective difference of chroma by the consensus of our three observers. We used Hering R, baryta W, and velvet Bk papers. We determined the tint of the R by the method of constant stimuli (100 series, 5 stimulus pairs): 48W + 312Bk. (A flicker-photometer result is 57W + 303Bk.) Velvet Bk, under our conditions, had a light value of about .05 baryta W (Kirschmann photometer). From these values we could compute the W.V. and hence the saturation of every combination. The background for the matches were arranged in accordance with the W. V. of the inducing colors. In the description of the following nine inducing colors, D means much darker; d, darker; S, standard; l, lighter; L, much lighter; g, slightly better chroma than S; p, slightly poorer chroma than S; C. V., color-valence; W. V., white-valence; saturation, C. V./W. V.

			C.V.	W.V.	Saturation
1.	Dp:	60R + 2W + 298Bk.....	60	27	2.22
2.	Dg:	70R + 285Bk.....	70	27	2.78
3.	dp:	130R + 15W + 215Bk.....	130	49	2.65
4.	dg:	150R + 12W + 198Bk.....	150	49	3.06
5.	S:	180R + 24W + 156Bk.....	180	64	2.82
6.	lp:	250R + 33W + 77Bk.....	250	82	3.05
7.	lg.	270R + 30W + 60Bk.....	270	82	3.29
8.	Lp:	300R + 47W + 13Bk.....	300	102	2.94
9.	Lg:	315R + 45W	315	102	3.09

All nine colors were used with *W* and *C*. *B* worked with S and the extreme pairs only.

With every inducing color we began with a ring of 18°W and, after matching the contrast-color of the ring by the disks on the second mixer, increased the W in the ring by 30°; and so on, by 30° steps, until in two successive matches the observer required no color on the second motor.

The contrast-color of the ring was matched by baryta W, velvet Bk, Hering B, and Hering G. The C. V.'s were taken as the sum of the degrees of G + B. As the proportion of G to B was fairly constant, no great error was involved.[15] In computing the W. V. of the match of every ring we had to rely on flicker-photometer equations for B and G: G = 148 W + 212 Bk; B = 33 W + 327 Bk. Since our data are rough, any error involved in this method of photometry is probably not serious. The saturations of the induced contrast-color in every ring were computed from these values of C. V. and W. V.

[15]The av. ratios of G to B and their M. V.'s, where 10° or more of color were used, are: for *W*, 1.27 ± .15; for *C*, 1.16 ± .30; for B, 1.26 ± .18. Cook and Kunkel added degrees of G and B; p. 18.

The C. V.'s are shown in Table I; the saturations in Table II. The tendency in both cases is for the values to start, with the darkest ring, at a minimum, rise to a maximum, and then drop off to zero with the lighter rings. Hering R is a dark paper; hence we were unable to get a range of darker rings as great as the range of lighter rings. Our darkest ring (18° W + 342° Bk; W. V. = 35) was actually lighter than our darkest colors (Dg, Dp; W. V. = 27). If we had foreseen this difficulty we could have used a ring of 360° Bk (W. V. = 18) or possibly a lightless hole (W. V. = 0); but even these stimuli would not have given symmetry.[16]

TABLE I

COLOR-VALENCES

Obs.	Degrees of W in contr. ring	Inducing Colors								
		Dp	Dg	dp	dg	S	lp	lg	Lp	Lg
	18	20	19	0	8	6	0	8	0	0
	48	0	7	22	23	26	24	28	15	15
	78	0	0	7	7	15	31	22	24	21
	108		0	0	3	0	15	18	22	34
W	138			0	0	0	8	0	20	33
	168				0		0	0	20	13
	198						0		0	3
	228								0	0
	258									0
	18	5	9	7	9	5	5	10	5	0
	48	5	4	16	12	19	22	17	19	15
	78	0	0	7	6	14	22	15	34	32
	108	0	0	0	3	11	19	14	35	35
C	138			0	0	14	7	15	7	15
	168			0	0	11	0	13	0	5
	198					22	0	13	0	0
	228					0		9		0
	258							0		
	18	15	11			28			0	15
	48	10	9			49			23	33
	78	0	3			44			46	55
	108	0	0			25			61	46
B	138		0			33			64	63
	168					0			47	44
	198					0			0	21
	228								0	0
	258									0

[16] One can not overcome the difficulty by partial light-adaptation; such a procedure would give darker R's as well as darker Bk's. The only solution seems to be a lighter colored paper, say Hering G or Y.

TABLE II

SATURATIONS

Obs.	Degrees of W in contr. ring	Inducing Colors								
		Dp	Dg	dp	dg	S	lp	lg	Lp	Lg
	18	.39	.37	0	.23	.23	0	.19	0	0
	48	0	.10	.30	.30	.36	.39	.45	.24	.22
	78	0	0	.07	.07	.16	.34	.37	.26	.23
	108		0	0	.02	0	.14	.15	.20	.30
W	138			0	0	0	.06	0	.13	.23
	168				0		0	0	.10	.08
	198						0		0	.01
	228								0	0
	258									0
	18	.11	.19	.17	.22	.14	.14	.26	.15	0
	48	.07	.05	.21	.17	.26	.33	.27	.32	.24
	78	0	0	.08	.07	.15	.26	.18	.38	.34
	108	0	0	0	.02	.09	.20	.12	.34	.30
C	138			0	0	.10	.05	.12	.05	.12
	168			0	0	.06	0	.09	0	.03
	198					.10	0	.07	0	0
	228					0		.04		0
	258							0		
	18	.33	.24			.64			0	.47
	48	.14	.12			.65			.35	.51
	78	0	.03			.42			.56	.60
	108	0	0			.21			.58	.46
B	138		0			.21			.46	.46
	168					0			.28	.29
	198					0			0	.12
	228								0	0
	258									0

TABLE III

MAXIMAL COLOR-VALENCES AND SATURATIONS INDUCED BY DIFFERENT INDUCING COLORS

Inducing Color	Dp	Dg	dp	dg	S	lp	lg	Lp	Lg
W.V	27	27	49	49	64	82	82	102	102
Saturation	2.22	2.78	2.65	3.06	2.82	3.05	3.29	2.94	3.09
Maximal C.V. Observed:									
W	20	19	22	23	26	31	28	24	34
C	5	9	16	12	22	22	17	35	35
B	15	11			49			64	63

TABLE III—*Continued*

Maximal C.V. Computed:									
W..............	15	18	16	17	25	27	25	27	29
C..............	6	9	16	10	16	24	16	31	29
B..............	16	12			43			61	58
Maximal Saturation Observed:									
W..............	.39	.37	.30	.30	.36	.39	.45	.26	.30
C..............	.11	.19	.21	.22	.26	.33	.27	.38	.34
B..............	.33	.24			.65			.58	.60

Table III shows the maximal amounts of induction occurring for every inducing color. The observed maximal C. V.'s from Table I are first tabulated. In some cases, however, the data of Table I do not indicate clearly the amount of the maximum; the function is irregular, and we wished that all points might be brought to bear upon the amount and position of the maximum. Accordingly we smoothed out the curves by calculating, by the method of least squares, the most probable parabola; and in this way determined the computed maximal C. V.'s of Table III.[17] Finally, Table III gives the observed maximal saturations. These values are indicated unequivocally in Table II; hence it was not necessary to generalize the curve in accordance with an hypothesis.[18]

Table III shows that the maximal C. V.'s tend in general to increase with the brightness (W. V.) of the inducing color. The computed values, which are based on three to nine points instead of a single one, show this tendency more strikingly. Cook and Kunkel's law appears to hold with the C. V.'s. With the saturations, however, it does not hold. It is true that there is evident with the saturations some slight tendency for increase, but this tendency appears to be due to the differences in saturation of the inducing colors. The darker colors unfortunately turned out to be the less saturated. The correlations between the saturations of the inducing colors and the maximal induced saturations run about 60 to 70%.[19]

[17]The details of the computation can not be given here. We used the parabola, $y = Ax^2 + Bx + C$, because a preliminary investigation showed that it approximately fitted the average of the data if the extreme points were omitted. The normal equations are:

$$[x^4]\,A + [x^3]\,B + [x^2]\,C = [x^2y]$$
$$[x^3]\,A + [x^2]\,B + [x]\,C = [xy]$$
$$[x^2]\,A + [x]\,B + nC = [y]$$

Then the brightness of the ring suffering maximal contrast is $x_m = -B/2A$. The required maximal ordinate is $y_m = C - Ax_m^2$. The data for Dp and Dg do not show a maximum; hence for them we used the parabola, $y = Ax^2 + C$, where x_m is assumed 0, and $y_m = C$.

[18]If a parabola be passed through the three maximal points (*cf.* F. M. Urban, *Statistical Methods,* 1908, 124f.), the maximal values are not altered within our rough limits of precision.

[19] For *W* the correlation, r, = 10.2%; for *C*, r = 76.0%; for *B*, r = 59.6%. But if for *W* the two rather unreliable values for Dp and Dg be discarded, then his r = 61.8%.

Hence we may conclude that such significant variation of the saturations as exists is due to the saturation of the inducing colors and not to their tint.

Table IV gives the tints (W. V.'s) of the gray rings in which the maximal C. V.'s and saturations were induced. The points for the C. V.'s were found by the parabolic interpolation mentioned above. The points of maximal saturation were computed by an interpolation in which the bell-shaped curve of error was used as an hypothesis.[20] There are some wide deviations, but in general the tints undergoing maximal induction increase with the tints of the inducing colors, thus establishing approximately the principle of Kirschmann's law. The data for the saturations accord with the law about twice as well as the data for the C. V.'s.[21]

TABLE IV

WHITE-VALENCES (TINTS) OF THE GRAY RINGS IN WHICH THE MAXIMAL COLOR-VALENCES AND SATURATIONS WERE INDUCED BY THE DIFFERENT INDUCING COLORS

Inducing Color	Dp	Dg	dp	dg	S	lp	lg	Lp	Lg
W.V.	27	27	49	49	64	82	82	102	102
W.V. of Maximal C.V. (computed):									
W	< 35	< 35	75	68	73	88	83	125	119
C	35±	< 35	64	55	122	92	124	101	110
B	< 35	< 35			84			125	128
W.V. of Maximal Saturation (computed):									
W	< 35	< 35	64	56	63	65	72	86	108
C	< 35	< 35	57	35	61	74	38	82	91
B	< 35	< 35			48			114	90

Finally we note, in so far as our scanty data permit us, that there is no evidence of asymmetry of function about the point of maximal induction. If we arrange all our data with the maxima coincident (omitting Dp and Dg) and average the coincident values, we get for the C. V.'s an average function:

2, 7, 16, 27, 27, 16, 5, 1, 0;

and for the saturations an average function:

.19, .26, .36, .25, .13, .06, .02, .01.

We may conclude then:

(1) That light colors exhibit no marked tendency to induce more saturated contrast-colors than do dark colors;

[20] Both the C. V.-function and the saturation-function tend to be bell-shaped. We adjusted the saturations to the curve of error by omitting all zeros and assuming the maximum of the curve to be the observed maximum. We could not, therefore, use the curve of error with the C. V.'s, because with them we wished also to determine the amount of the maximum. The curve of error is, however, the more representative hypothesis and is the easier to apply. The normal equations are similar to those of the method of constant stimuli.

[21] When agreement is measured by the square root of the sums of the squares of the deviations.

(2) That light colors do induce contrast-colors of a greater color-valence, but that this greater color-valence is approximately proportional to the greater color-valence which, in relation to their saturation, the light inducing colors themselves possess;

(3) That maximal saturations are induced in grays of approximately the same brightness as the inducing color (Kirschmann's law), and that the induced saturation falls off to zero in an approximately symmetrical function with increased brightness-contrast in either direction;

(4) That, less accurately stated, maximal color-valences are induced in grays of approximately the same brightness as the inducing color;

(5) That equal saturations, determined as the quotient of color-valence by white-valence, are at least approximately equivalent to subjectively equal chromas, and may be used in a rough way to indicate equal chromas; and

(6) That rough functions, such as we have determined, can be made to yield more general results, if they are taken as wholes by adjustment, by the method of least squares, to some general hypothesis.

Ebbinghaus' explanation of Kirschmann's law, namely, that the fading out of color in the light grays is due to the desaturating effect of the white and the fading out in the dark grays to irradiation, does not appear, on these conclusions, to be valid. Ebbinghaus would lead one to expect that with the light gray rings the color-valence would remain about constant, the white-valence would increase, and the saturation would consequently decrease, eventually, no doubt, to a subliminal value. But we have seen that the color-valence falls off quite as rapidly as the white-valence increases. Furthermore, Ebbinghaus' supposition that different causes are operative at the two extremes of brightness-contrast would make an asymmetrical function probable; whereas our data indicate, though but roughly, that the function is symmetrical.

BOOK REVIEW

The life and work of George Sylvester Morris. By R. M. WENLEY. New York, Macmillan Co., 1917. 332 p.

The author is the successor of Professor Morris, who died twenty-five years ago, and this book is a labor of love. The author has taken endless pains to investigate everything connected with the subject of this memoir. A more exhaustive biography, and we may add a more appreciative one, has rarely been written. George Morris was a pioneer student of the history of philosophy in this country, a careful though not so very voluminous author, a musician of rare talent, who throughout his life gave great attention to this art, the translator of "Ueberweg," to which he added a great deal of material of his own. He might be in a new sense called a spiritual man. The Hegelism of his earlier and the Kantianism of his later life he had made so intimate a part of his own personality that it was very hard for him to discuss such themes with those whose standpoint differed from his own. To have traced the Morris lines back to the Pilgrims, to have dug up the details of his New England home, school and college life, early manhood, his experiences in the war, at Dartmouth, at Union Theological Seminary, at John Hopkins and Michigan, where most of his later life was spent, to have traced the origin, transition and final stage of his intellectual history and to have depicted the man and teacher so vividly, is a real service to the history of culture in this country. One can truly say that of all the many good fortunes that could befall such a man, the greatest is to have so worthy, eminent and loyal a biographer. Morris himself was naturally modest and retiring. He was in some sense an esthetic soul, who loved philosophy because of the personal satisfaction he derived from this study. Moreover, he lived at a time when in this country his studies met with very scant appreciation, save by the few, so that much of the best that he did and wrote had already begun to be forgotten, and it is a splendid act of mere justice, to say the least that can be said of it, to set his life and doctrine forth in the new, fresh way in whcih they are here presented. Possibly (and this is the only criticism that anyone could make of his work) the author at times takes too much pains and goes into too minute details, e. g., concerning Morris' early life and relatives. But in a work written in the environment in which Morris so long lived, and in closest contact with the surviving members of his family, as it was, this cannot be called a blemish. The writer of this note cannot recall the name of a single American thinker who has had so splendid a biography. Indeed, this is so good that the name of Morris will always be associated with that of Wenley. The book ought to inspire others who have the now rather dwindling faculty of appreciation, to give us similar memoirs of James, Royce, Münsterberg, Charles Pierce, and perhaps we might add William T. Harris; but nothing of any of these or others who might be named has ever been attempted on such a scale.

BOOK NOTES

General types of superior men. By OSIAS L. SCHWARZ. Boston, Richard G. Badger, 1916. 435 p.

This is a philosophical, psychological study of genius, talent and Philistinism in their bearings upon human society and its struggles for a better social order. It has a preface by Jack London and an introductory letter from Max Nordau. The main topics are, genius, talent, superior and average minds, adaptation, heredity and variability, origins and causes, conditions and incentives, types of superior men, intellectual hierarchy, classification, creative, affective, striving life, influence on the masses, affectation, normality. Max Nordau writes that the book is teeming with ideas but still more seething with feelings. It is vehement, preaching in the Old-Testament, prophetic style, abrupt, fitful, violent, and the study of genius is really a pretext for invective against the average man, who is in fact an average beast. The author does not stop to prove and Nordau says that his picture of real society, which is probably American, is ghastly and appalling. Nordau tells him his command of English is wonderful.

The fundus oculi of birds, especially as viewed by the ophthalmoscope. A study in comparative anatomy and physiology. By CASEY ALBERT WOOD. Chicago, Lakeside Press, 1917. 180 p.

This impressive quarto volume begins with a summary of conclusions, bibliography, and a review of the anatomy and physiology of the organs and tissues seen in the fundus. The author next gives us an outline of ophthalmoscopy, and then proceeds to describe his application of its methods to the fundus of living birds, the eye-ground in general, the pecten and optic disc, the macular regions, foveal blood-vessels, nerve fibres, choroid and retina. He has also succeeded in giving us some very good photographs of the fundus in living birds. An interesting section describes the effects of domestication upon it. He tells us how it differs in different orders of birds, and how it may be used as a basis of classification; discusses the relations between the reptilian and avian fundi. The text itself is illustrated by 145 cuts, and to these are appended 61 full-page colored paintings.

Nichiren, the Buddhist prophet. By MASAHARU ANESAKI. Cambridge, Harvard University Press, 1916. 160 p.

Religious psychology has of late been extended by the development of considerable new material, either the expressions or the studies of strong personalities. The author was encouraged to write this book by the late Professor Royce. The material is remarkably copious, considering that this great Buddhistic reformer lived in the thirteenth century, but this is partly due to the revival of his teachings and spirit in modern Japan. The first chapter describes his time; the second, his birth, studies, conversion, and the Lotus of Truth. Then in successive chapters follow his public appearance and persecution; an interlude and a narrow escape; the threatening Mongol invasion and the sentence of death; the exile in Sado and the ripening of

Nichiren's faith in his mission; the climax of his life, the graphic representation of the supreme being; release and retirement; a paradise on earth; silent prayer and anxious watching; the last stage of his life and his death. To this is appended a chapter on the Buddhist conception of reality.

Science and learning in France; with a survey of opportunities for American students in French Universities. An appreciation by American Scholars. Society for American Fellowships in French Universities, 1917. 454 p.

This is a coöperative volume designed to show Americans the contributions of France in all branches of scientific knowledge, and to show her status in the forefront of the world's progress, and especially to give American students information as to graduate work in France. Each chapter (and there are over a score of them, some with various sub-divisions) takes up a particular topic, beginning with anthropology, archaeology, astronomy, etc., and gives briefly its history and prospects. President Charles W. Eliot and Dr. George E. Hale write introductions, and then follow the contributions of some two-score authors, with a long list of some thirty double-column pages of sponsors. It is designed to be a token of national homage on the part of America to France. While most heartily commending the purpose of such a work, it must be admitted that the chapters on the different topics are not, as a whole, very luminous. Some of them particularly are very perfunctory, superficial, ill-informed, and do scant justice to their subject. A great deal of the space of most is devoted to the great men of the past, and in the present writer's estimation too little attention is given to the actual present-day opportunities. It is doubtful whether this book would really contribute much in the way of either informing or inclining young graduates to go to France.

The growth of medicine from the earliest times to about 1800. By Albert H. Buck. New Haven, Yale University Press, 1917. 582 p.

The writer regrets that most medical schools give no teaching in the history of medicine but thinks two good reasons are the newness of the country and the few and rather too scientific character of texts. The latter trouble he seeks to obviate, and for those wanting something more exhaustive he refers to a more comprehensive work by Max Neuberger, of which two volumes have already appeared. He also refers to Haeser. The preface makes no mention of Sprengel's voluminous work in seven volumes, and although it is included in the author's literary references, he seems to have made very little use of it, and from the standpoint of the present reviewer he has hardly done justice to the Arabic influences in medicine. The chapters are divided into three groups, ancient, mediaeval and renaissance. The last or fourty-third chapter, describes the first appearance of syphilis in Europe.

The sex worship and symbolism of primitive races. By Sanger Brown II. Boston, Richard G. Badger, 1916. 145 p.

This is an interesting epitome of the main points and authors, old and new, in this field, from Jennings, Knight, Inman and Spencer, down to Frazer, Howitt and Miss Harrison. It contains four chapters, on simple sex worship, symbolism, sun myths, mysteries and decadent sex worship, and interpretations. The book is a very convenient epitome. The chief criticism of it, however, is that it seems to have

been written almost from start to finish in a pre-Freudian age. Only in the latter part of the book do we learn that the author has heard of this epoch-making man and his group of co-workers in a line in which this book really helps us on.

Group theories of religion and the individual. By CLEMENT C. J. WEBB. New York, Macmillan Co. (c. 1916). 208 p.

This work is devoted to an explanation of Durkheim and Lévy Bruhl, and as these views are very much in the forefront of discussion nowadays, this resumé is most opportune. There is a good deal of exposition of Durkheim, which is well, although some would perhaps wish that there had been more of this and less of the criticism of Mr. Webb. The chief topics are, the laws of contradiction and participation; Durkheim's definition of religion; criticism of his social theories of it; the social theory of categories; of pre-logical mentality; philophical basis of the theories of the sociologists; Durkheim's philosophy of religion; group theories and individual theories of religion.

Comparative religion; its adjuncts and allies. By LOUIS HENRY JORDAN. Oxford University Press, 1915. 575 p.

Part I. disucsses the avenues of approach, (1) anthropology, (2) ethnology, (3) sociology, (4) archaeology, (5) mythology, (6) philology, (7) psychology, (8) the history of religions, divided into special groups. Part II. the transition, describes the evolution of scientific method, apologetic treatises, translations of sacred texts, transactions of congresses, societies, encyclopedias, etc., special works, periodical centers of study. Part III. discusses comparative religion, its restricted idea and its legitimate scope. This is the most comprehensive attempt to depict the rise, sources, transitions, capacities and issues of this great department which does not appear now to be growing rapidly.

Gleanings from old Shaker journals. Compiled by CLARA ENDICOTT SEARS. Boston, Houghton Mifflin Co., 1916. 298 p.

The Shaker village at Harvard, Massachusetts, is unique because it was the headquarters of Mother Ann Lee, when she was spreading her gospel through the eastern states in the eighteenth century. All this is of great psychological interest. The wall of reserve has hitherto made the Shakers almost inaccessible, but the old antagonism between them and the world has now pretty well died out, as have the religious excesses of the old days. Now they are peaceful, honest, and affectionately esteemed. The first few chapters deal very largely with Ann Lee and her associates, and the later chapters bring the story down to the present time. Altogether it is a very interesting study, well worth while.

The spiritual ascent of man. By W. TUDOR JONES. New York, G. P. Putnam's Sons, 1917. 247 p.

This book seeks to bring the best philosophical thought of the time within the range of the ordinary reader, and is addressed to Englishmen. The author wants to create an interest in things of the spirit in the average man, believes there is no limit to the possibility of spiritual development, that the ideal is close at hand, and the divine is possible, life cannot be reduced to matter. The chief chapters treat of the scope and limit of science, matter and life, body and mind, the intellect and nutrition, the "is" and the "ought," values, the nature of the spirit, conception of God and Christianity.

Is civilization a disease? By STANTON COIT. Boston, Houghton, Mifflin Co., 1917. 136 p.

The question raised by this book has been often put before, and we have even of late heard the question discussed whether Christianity is a disease, unethical, bad or wrong. Edward Carpenter long ago published a work, once widely read, entitled "Civilization, Its Cause and Cure." The author stresses the gap between Christ and his modern worshipers. The main point, on which this work is rather sentimental, and not a very scholarly group of aphorisms, is that conventions—business, religion—of modern life do not fit man's nature and must be moulted, and he must make a new start.

Rest days: a study in early life and morality. By HUTTON WEBSTER. New York, Macmillan Co., 1916. 325 p.

This is a very interesting and learned anthropological study of the observance of days of abstinence, quiescence, often due to taboos. These are placed on days and at critical epochs by different races, also after a death. Then come chapters on holy-days, market-days, lunar superstitions and festivals, calendars and the week, Babylonian evil days, the Hebrew Sabbath, and unlucky days. The discussion of all these topics is not only learned, with countless references to original authorities, but is broad and philosophical.

Bells and bell lore; church bells of Amounderness and the Archdeaconry of Lancaster. By T. HARRISON MYRES. Preston, Guardian Printing Works, 1916. 112 p.

This fascinating theme is treated with great detail. The material is divided into two parts. The first is the general history of bells in different countries and cities, and the origin of certain customs, while the second part is occupied chiefly with accounts of bells in different English churches, together with traditions connected with them. The book is illustrated by many small cuts and a number of full-page illustrations and infolds. It it a work of details with little or no attempt at generalization.

Modern man and his forerunners; a short study of the human species living and extinct. By H. G. F. SPURRELL. London, G. Bell and Sons, 1917. 192 p.

This is an interesting and timely compilation. The author first lays down some general principles of anthropology and discusses the zoological position of man, extinct species and races of man and their culture, the growth of human powers and habits from the neolithic age, the origins of civilization, its growth and spread, and finally man to-day.

The way to Nirvana; six lectures on ancient Buddhism as a discipline of salvation. Hibbert Lectures, Manchester College, Oxford, February-April, 1916. By L. DE LA VALLÉE ROUSSIN. Cambridge, University Press, 1917. 172 p.

This work is in six chapters, (1) Origins of the Indian Disciplines of Salvation; (2) the Buddhist Soul; (3) Buddhist Definition of Karman; (4) The Doctrine of Karman and Transmigration, Cosmogony, Theogony; (5) Nirvāna; (6) The path to Nirvāna.

The holiness of Pascal. By H. F. STEWART. (The Hulsean Lectures, 1914-15). Cambridge, University Press, 1915. 145 p.

These lectures are first, biographical; second, Pascal in controversy; third, his doctrinal system; and fourth, his personal religion.

613

INDEX OF SUBJECTS

(The subject headings of all contributions except Book Reviews and Notes are printed in SMALL CAPITALS.)

INDEX OF SUBJECTS

615

INDEX OF AUTHORS

(The names of authors of original contributions are printed in SMALL CAPITALS.)

616.

INDEX OF AUTHORS

The American journal of psychology

8